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The Endogeneity Bias in the Relation between Cost-of-Debt Capital and Corporate Disclosure Policy.[§]

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Abstract

The purpose of this paper is twofold. First, we provide a discussion of the problems associated with endogeneity in empirical accounting research. We emphasize problems arising when endogeneity is caused by (1) unobservable firm specific factors and (2) omitted variables and discuss the merits and drawbacks of using panel data techniques to address these causes. Second, we investigate the magnitude of endogeneity bias in Ordinary Least Squares regressions of cost-of-debt capital on firm disclosure policy. We document how including a set of variables which theory suggests to be related with both cost-of-debt capital and disclosure and using fixed effects estimation in a panel dataset reduces the endogeneity bias and produces consistent results. This analysis reveals that the effect of disclosure policy on cost-of-debt capital is 200% higher than what is found in Ordinary Least Squares estimation. Finally, we provide direct evidence that disclosure is impacted by unobservable firm-specific factors that are also correlated with cost-of-capital.

JEL-code: M41; G3; C23
Keywords: Disclosure policy; cost-of-debt capital; endogeneity

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The Endogeneity Bias in the Relation between Cost-of-Debt Capital and Corporate Disclosure Policy.

Introduction

Corporate disclosure policy is one of the most widely researched topics in accounting. Theory has generally suggested a *negative* causal relation between the quality of information disclosed by a firm and its cost of capital (Verrecchia, 2001; Dye, 2001; Easley and O'Hara, 2004). The basic idea is that disclosure reduces both the information differences and incentive problems between the firm and its investors (Healy and Palepu, 2001). Investors, then, 'reward' firms for high-quality disclosures with lower required returns.

In recent years, however, both the existence and sign of the relation between disclosure and cost-of-capital has been called into question not in the least because the empirical literature has provided conflicting results. While some studies find strong negative associations consistent with theoretical predictions (Welker, 1995; Leuz and Verrecchia, 2000; Sengupta, 1998), other fails to document a significant relation (Botosan and Plumlee, 2002; Botosan and Frost, 1998), find only partial evidence (Botosan, 1997; Healy et al., 1999; Richardson and Welker, 2001) or even report a positive association (Heiflin, Shaw and Wild, 2003).

Some commentators have pointed to the possibility of endogeneity bias as a potential explanation why empirical findings are not consistent with theory and report contradicting results with regard to the sign of the relation (Healy and Palepu, 2001; Core, 2001; Zhang, 2001).[1] It is well know that endogeneity causes Ordinary Least Squares regressions to be biased and inconsistent (Wooldridge, 2002). Findings from OLS regressions of cost-of-capital onto disclosure are difficult to interpret in the presence of endogeneity and this may very well account for the lack of agreement in the empirical literature on the sign of the relation.

[1] Other potential explanations for these conflicting results are the current high standards of mandatory disclosure (rendering voluntary disclosure choices of second order importance) and measurement problems in the somewhat elusive key constructs of 'information problems' and 'disclosure quality' (Leuz and Verrecchia, 2000; Healy and Palepu, 2001; Zhang, 2001).

We document the effect of endogeneity bias on the relation between disclosure and cost-of-debt capital. We define endogeneity bias broadly as any situation where the disturbance term of the structural equation is correlated with one or more independent variables.[2] Intuitively, our reasoning is that differences exist in the cost of debt that are correlated with the firm's disclosure policy, but that are not necessarily caused by this policy. Instead, these differences are caused either by (1) unobservable heterogeneity among firms in a cross sectional sample or (2) observable determinants of cost-of-debt capital which are correlated with disclosure but omitted from the analysis. Note that these two sources of endogeneity bias are both variations of the correlated omitted variable problem and are in fact theoretically equivalent. To an empirical researcher they are different, however, because the first source is unobservable and should be roughly constant over time, while the second is observable and may change over the period of investigation. We will provide an illustration of both sources of endogeneity bias in turn.

One example of unobserved heterogeneity is the difference in 'costs of disclosure'[3] among firms. High costs of disclosure will reduce the optimal level of disclosure and at the same time increase the equilibrium cost-of-capital (Zhang, 2001). While in a cross sectional analysis, it will appear as if disclosure is causally related to cost-of-capital, what we observe in fact are equilibrium changes of both disclosure level and cost-of-capital each caused by the unobservable firm-specific characteristic of 'costs of disclosure'.

At least some of the determinants of a firm's disclosure choice would appear to be also related to the default risk of the firm (Jaffee, 1975, Kidwell et al. 1984; Fung and Rudd, 1986), and as such impact on the cost-of-debt.[4] For example, larger firms are generally

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[2] This definition is consistent with the econometrics literature (Greene, 2000; Wooldridge, 2002) and with the proposal in Chenhall and Moers (2004).

[3] Often these costs of disclosure are defined to include the costs of collecting, processing, reporting and verifying information and the cost due to loss of competitiveness (see, e.g., Wagenhofer, 1990; Guo, Lev and Zhou, 2004). Potentially interesting definitions also refer to the costs associated with uncertainty about investor reactions to a certain disclosure (Fishman and Hagerty, 2003; Verrecchia, 2001) or litigation costs (Skinner, 1997).

[4] Within standard asset pricing models, such as the CAPM, only undiversifiable risk is priced on the market, and therefore we have to assume that the proposed joint determinants of 'cost-of-debt capital', such as the firm's default risk, are at least partly correlated across firms. Indeed, an often-heard critique on studies that relate disclosure to cost of capital is that differences in disclosure quality are

considered less risky and therefore enjoy lower cost-of-debt capital (Fama and French, 1992, 1993). Larger firms also benefit from economies of scale in producing information. They usually have specialized departments set up to deal with investors' information needs and it will generally be less costly for them to compile more information and disclose it to the capital market. Empirically, size is significantly correlated with disclosure in many studies. In sum, size is associated both with cost-of-debt and with disclosure. When omitted from the analysis, one may find a negative relation between cost-of-debt and disclosure policy, but this association is likely driven by firm size.

After a brief review of the econometrics of endogeneity, we discuss in more detail the sources of endogeneity bias in the relation between disclosure and cost of capital. We then document empirically the effect of endogeneity bias in regressions of cost-of-debt capital on disclosure policy. Specifically, we use Sengupta's (1998) original model[5] as a starting point of our analysis and replicate this study's results in a sample similar to his. As in Sengupta, we establish a strong negative association between disclosure and cost-of-debt capital. We then augment Sengupta's model with variables that are known to be associated with a firm's disclosure policy and which are likely to affect cost-of-debt capital in order to address the endogeneity bias caused by omitted variables. Our results show that the coefficient on disclosure is reduced to approximately 50% of its former magnitude in the benchmark model and disclosure is no longer significantly related to cost-of-debt capital in the augmented version of our regressions. The omitted variable effect seems substantial.

Next, we evaluate both sources of endogeneity bias at the same time and use panel data techniques to estimate the augmented model. We find that once observable determinants

idiosyncratic and therefore should not 'survive the forces of diversification' (Leuz and Verrecchia, 2005: 1) nor impact on the cost-of-capital. Leuz and Verrecchia (2005), in contrast, argue that disclosure improves the coordination between the firm and its investors with respect to capital investment decisions. As such, poor disclosure quality can lead to misaligned investments and higher cost-of-capital. Other studies have suggested that disclosure may impact on cost-of-capital, even if it is idiosyncratic, because it improves market liquidity (Verrecchia, 2001; Leuz and Verrecchia, 2000), reduces estimation risk (Barry and Brown, 1985) or increases the investor base (Merton, 1987).

[5] Sengupta's model provides a convenient vehicle to illustrate the effect of endogeneity bias in disclosure research. It is also to some extent an arbitrary choice since endogeneity bias is present in many contexts in (financial) accounting research and many potential candidates exist for similar analysis as is conducted in this paper. Chenhall and Moers (2004), Ittner and Larcker (2001), and Larcker and Rusticus (2005) provide helpful discussions of endogeneity in accounting research.

of disclosure and cost-of-debt capital are included in the regression and the estimation technique controls for firm-specific effects, we re-establish the negative association between disclosure and cost-of-debt capital. The association is stronger than before and the difference is economically significant – the fixed effects coefficient on disclosure is over 200% larger than the OLS coefficient in the same model – which suggests that the cost-of-capital benefits of increased disclosure are much larger than previously thought and economically significant. Based on these analyses, our beliefs about the existence of endogeneity bias in the benchmark model are reinforced. We then suggest a simple procedure to directly assess whether the independent variables in the regression (in particular, the disclosure policy variable) are associated with unobservable firm heterogeneity and document that, in fact, disclosure policy is strongly positively correlated with firm heterogeneity.

Synthesizing our findings, we show that at the level of the individual firm, increases in disclosure are causally[6] associated with lower cost-of-debt capital. However, in cross-sectional analyses that do not control for endogeneity bias, a negative association between these two variables should not be interpreted causally and is likely caused by firm heterogeneity effects, which are compounded in the disclosure variable. The resulting association between disclosure and cost-of-capital is (at least partly) spurious.

Together these results speak strongly in favor of dealing explicitly with endogeneity when investigating the relation between disclosure policy and cost-of-capital. Note that while endogeneity has been identified as the 'most important limitation' (Healy and Palepu, 2001, 430) of disclosure studies, few attempts have been made to address the issue empirically (Cohen, 2003).

The remainder of this paper is organized into six sections. Section 2 provides a self-contained discussion of the econometrics of endogeneity bias in the context of financial accounting research. Section 3 discusses firm heterogeneity and correlated omitted determinants as two sources of endogeneity bias in the relation between cost-of-debt capital

[6] We recognize that causal statements cannot be made based on statistical considerations, but only on theory. When we refer to a causal relation, we use this as shorthand for 'a causal relation as suggested by theory and underpinned by empirical evidence'.

and disclosure. Section 4 outlines the research design and provides the variable definitions. Section 5 describes the sample and some summary statistics. Section 6 presents the empirical results on the extent of endogeneity bias in the association between disclosure and cost-of-debt capital. The final section summarizes the results and discusses the limitations to our analyses.

2. A note on endogeneity

The traditional textbook definition of endogeneity we used so far requires the disturbance term in the structural equation to be correlated with one or more explanatory variables. This rather arcane definition is not very helpful to applied researchers. We therefore propose a more intuitive definition (following Heckman, 2000), which is closer to the practice of economists. Economics "undertakes to study the effect which will be produced by certain causes, not absolutely, but subject to the condition that other things are equal and that causes are able to work out their effects undisturbed" (Marshall 1961, p. 36). Researchers aim at identification of these causal effects, which is done by measuring the effect of a certain cause while holding all the other causes in the model constant. This in itself is not a straightforward task since many causes will not vary independently. Our intuitive definition of endogeneity then is any situation where the *ceteris paribus* condition is not fulfilled whenever the independent variable of interest is changed.

Empirical researchers typically use an economic model or informal reasoning to arrive at a structural model, which represents the causal relations between the variables of interest. Although theory or earlier empirical work will often suggest that many of these variables cannot be said to be truly exogenous, empirical researchers will have to assume some are, to estimate the parameters of the structural model. A careful justification of why certain variables are exogenous is therefore required. In his presidential address, Demski (2004) advocates to explicate the micro foundations (preferences, expectations) of the choice behavior of economic actors in the relation under study and to apply equilibrium reasoning to derive a structural model. Such procedure allows for a better understanding of how all the salient aspects of behavior, such as causal effects, are captured into the model.

Suppose an empirical researcher is interested in the following structural model:

$$y = \alpha_1 x_1 + \alpha_2 x_2 + ... + \alpha_k x_k + u \qquad \text{(Equation A)}$$

where y, x_1, x_2, … x_k are *observable* random scalars and u is the *unobservable* random disturbance. An explanatory variable x_j is said to be endogenous *in equation A* if it is correlated with the disturbance term u; x_j is exogenous if it is uncorrelated with the disturbance term. It is important to stress that in this 'empirical' or econometric definition, variables are inherently neither exogenous nor endogenous; instead their nature is conditional on the way the structural model is written (Greene, 2000). An empirical researcher will be interested in estimating the parameters in the structural model. It is important to the researcher to know whether an explanatory variable is endogenous in a specific structural equation because it affects the way in which its parameter should be estimated. The upshot of all this is that it is paramount to be careful when using the words 'endogenous' or 'exogenous', since these designations are context-specific. The litmus test of the econometric form of endogeneity is whether the parameters of interest in the context of a specific structural model are affected by correlation between any explanatory variables and the disturbance term (Maddala, 2001). If they are the variable is said to be endogenous, if not it is exogenous. Since there is no clean-cut statistic or diagnostic instrument available to 'test' for endogeneity, the econometrics literature often advises empirical researchers to apply introspection (Wooldridge, 2002) or the criterion of reasonableness[7] (Greene, 2000; Kennedy, 2003) as a way to determine whether there is an endogeneity problem. It would appear that researchers are left rather vulnerable against allegations that their model suffers from 'endogeneity problems'. In the end, researchers have to determine which variables they care about (i.e., are the focus of their analysis) and should therefore be as free from bias as possible, and which variables they do not care about and are only in the model as a control. Bias in the estimates of the latter variables are less of a problem and should not be weighted to heavily when evaluating the soundness of empirical work.

[7] One test is that the choices made should be palatable to the researcher's peers.

Sources of 'econometric' endogeneity

The source of correlation between the structural disturbance and an explanatory variable is important because it provides clues how endogeneity can be addressed. Wooldridge (2002) lists three common sources of endogeneity: (1) omitted variables, (2) simultaneity and (3) measurement error. Our discussion will focus on the first two of these. Considerable advances have been made to mitigate measurement error in variables using latent variables techniques. While some of the methods to address endogeneity we discuss below may also reduce measurement error, the literature seems to move towards the use of these latent variables techniques (Larcker and Rusticus, 2005), and we defer further elaboration here. Note that each source of econometric endogeneity will affect the consistency of the estimation in a similar fashion and as such confound the interpretation of the regressions.

Omitted variables: causes

The first source of endogeneity arises if the structural disturbance term consists of omitted variables and these variables are correlated with one or more of the explanatory variables. This may occur because data is not available on those variables the researcher would like to include additionally into the model. These omitted variables are said to be unobservable to the researcher.[8] Omitted variables also may be due to a failure of the researcher to include all the *observable* factors theory suggest to be important in explaining the dependent variable. Economic relations are often such that two factors that are determinants of the same dependent variable will be mutually associated. If one such factor is omitted from the analysis and thus included in the disturbance term, the latter will be correlated with the included factor. One special case of omitted observable variables arises when the omitted variable is a function of an explanatory variable in the model. This type of omitted variable problem is often referred to as 'functional form misspecification'.

[8] While the disturbance term then includes variables that are unobservable to the researcher, these factors may very well be observable to the economic agent under study. Indeed, endogeneity arises when the explanatory variables represent decisions made by the agent on the basis of such factors (Hayashi, 2000).

In sum, omitted variables can be either observable or unobservable to the researcher. Omitted variables are captured by the disturbance term in the structural equation. When these omitted variables are correlated with explanatory variable x_i, then x_i is endogenous in that particular structural equation.

Omitted variables: potential 'solutions'

We emphasized that omitted variables may be either observable or unobservable to the researcher because this dimension matters when trying to mitigate the problems associated with estimating the parameters in the structural model. It should be noted that it is unlikely for any of the methods we describe to resolve fully the issues associated with endogeneity.

Omitted observable variables. This source of endogeneity can be addressed by including all factors that are important in explaining the dependent variable and, at the same time, are associated with one of the explanatory variables, into the structural equation. Factors that are associated with both dependent and one or more explanatory variables are said to be 'joint determinants'. In practical terms, this will usually require the researcher to conduct a thorough review of the extant theoretical and empirical literatures to identify these joint determinants. Once included in the structural model, the disturbance term is purged from the source of its correlation with the explanatory variables and the estimation of the parameters of interest should no longer be affected by endogeneity.

Omitted unobservable variables. Since the researcher will not be able to gather data on omitted variables that are unobservable, our earlier recipe of including any joint determinants will no longer work. We will discuss two distinct instances of omitted unobservable variables and methods to address these, which are relevant to the accounting literature, (1) self-selection and (2) firm-specific heterogeneity.

Self or sample-selection arises if the probability that a firm is included into the sample and the dependent variable are both affected by an (omitted unobservable) variable. As a result the sample is no longer random. Alternatively, the omitted unobservable variable may affect the way in which an observation is categorized within the sample, although all observations are included.[9] A good example in an accounting context is provided by Leuz and Verrecchia (2000). These authors study a sample of firms that have switched from a German to an international reporting regime. They are interested in the question whether a commitment to increased disclosure, as required under international standards, has tangible benefits in the form of lower cost-of-capital. Firms will decide on disclosure based on the expected consequences with regard to their cost-of-capital. Therefore, the factors that determine the disclosure choice (expected net cost-of-capital benefit) are likely to also affect the dependent variable, current cost-of-capital. Simply regressing cost-of-capital on disclosure would not do in this context because it ignores the fact that only those firms with positive expected net cost of capital benefits will have selected to switch reporting regime. As Leuz and Verrecchia are careful to point out, without discounting this *selection* effect the association between disclosure and cost-of-capital will be overstated for those firms that have switched regimes and understated for the firms that have not. Although, the expected net benefits of increased disclosure to the firm are unobservable to the researcher, they should be accounted for when estimating the structural model of interest. This is usually done by modeling the selection mechanism explicitly and adjusting the estimation of the parameters in the structural model for the selection effect. Heckman's (1979) procedure offers an often-used, easily implemented approach to achieve this.

Firm-specific heterogeneity. Unobserved omitted variables often represent features of the firm that are given and do not change over the period in question. Specifically, firm characteristics like managerial ability, structural arrangements, and employee skills can be thought of as roughly constant over time. As before, if these firm characteristics impact on both the dependent variable and one or more explanatory variables, the structural disturbance (which captures heterogeneity across units of observation) will be correlated with those explanatory variables. For example, more talented managers may prefer high-quality

[9] Self selection bias will also arise when the sample is truncated or censored, or sampling is on the dependent variable. When sampling is on one of the exogenous variables, the sample will not be random but estimation of the structural model is unaffected (Wooldridge, 2002). See also Shehata (1991) for a discussion of selection bias issues in an accounting context.

disclosures and, at the same time, the market may think these managers better 'risks' and charge a lower cost-of-capital. The talent of management is difficult to observe for a researcher and should be relatively constant. Regressions of cost-of-capital onto disclosure are affected by firm-specific heterogeneity bias if the talent of managers is not properly discounted.

Firm-specific heterogeneity can be addressed in several ways. Researchers may find a *proxy* variable for the firm characteristic and plug this into the structural equation. Alternatively, instruments might be available for those explanatory variables that are correlated with the unobservable firm characteristic and *instrumental variable* (IV) estimation can be used to estimate the parameters of the structural equation consistently (see, Wooldridge, 2002). Often, it will be the case that accounting researchers can observe a firm at different points in time. If so, panel data techniques are available to account for heterogeneity.

Since the choice of which method to use to address firm-specific heterogeneity directly impinges on our empirical work and is of practical concern in many other settings as well, we digress briefly from the main topic and discuss the tradeoffs involved when using IV versus panel data techniques.[10] Asymptotically, IV and fixed effects estimation must agree,[11] which makes it relevant to compare their properties in applied settings.[12] Panel data techniques address a narrower problem because they can only deal with time-invariant omitted variables. IV estimation does not assume that firm characteristics are constant and hence admits modelling the impact of a broader set of unobservable variables. Nevertheless, IV estimation is vulnerable to producing misleading results when the instruments used are not valid or weak. Instrument variables must be independent of the (unobservable) structural disturbance term and as highly correlated as possible with the explanatory variable they represent. The first condition cannot be tested; the second is frequently not met in practice

[10] This discussion is geared towards one panel data technique in particular: fixed effect estimation.
[11] If fixed effects and IV estimation do not agree, the implication is that the model is misspecified (e.g., the instruments are invalid or endogeneity is not alleviated by fixed effects estimation. A Hausman-type test may be used to discriminate between the estimators.
[12] It is not immediate which estimator will be more efficient asymptotically. This will depend on the number and quality of instruments and the amount of within-variation.

(Larcker and Rusticus, 2005). Not only is it often difficult to find valid and strong instruments in applied settings, the choice between alternative candidate instruments is subjective and may impact on the robustness of the empirical work.[13] Panel data techniques, on the other hand, are easy to implement and do not involve a subjective choice by the researcher. They assume, however, that the relation under study is essentially driven by changes within the firm, not by differences between firms. In other words, the cross-sectional variation should be limited compared to changes within firms. Since panel data techniques require multiple observations of a firm, the likelihood of a selection bias is higher than when IV estimation is applied. In sum, neither IV estimation nor panel data techniques dominate when trying to solve for endogeneity. The final choice between the two methods will depend on the specifics of the research design.

We conclude this section on omitted variables with an often-misunderstood fact. The mere fact that some variable represents a decision (or choice) to the firm or, more generally, an economic agent, is not in itself sufficient for 'econometric endogeneity' to arise. Only if the factors that impact on the decision by the economic agent, whether observable or nor, are also inter-related with the dependent variable will endogeneity exist.

<u>Simultaneity: causes</u>

In many settings of interest to accounting researchers, the data generating process is essentially such that variables are simultaneously determined and interdependent. Simultaneity arises when at least one of the explanatory variables is determined simultaneously along with the dependent variable (Wooldridge, 2002). If so, the structural disturbance and the explanatory variable will be correlated. Intuitively, one can think of simultaneity as describing *instantaneous feedback* relations among variables. An accounting example is provided in Welker (1995). This author is interested in the relation between disclosure policy and liquidity in equity markets. He notes that effective corporate disclosure will mitigate information problems in the market and thus increase liquidity. At the same

[13] It is often not immediate whether including more than one instrumental variable is beneficial in finite sample settings. See, e.g., Kennedy (2003) for a discussion. A Sargan (1958) - Hansen (1982) test is available to evaluate whether extra instruments should be used.

time, corporate disclosure may be influenced by the information differences between the firm and the market and thus by current liquidity. There is an 'equilibrium feedback mechanism' (Griffiths et al. 1993) operating on disclosure and liquidity to determine the equilibrium outcomes for both variables.

Simultaneity: potential 'solutions'

To capture instantaneous feedback relations, researchers write a system of equations that consists of separate structural equations for each endogenous variable. When variables y_1 impacts on y_2 and vice versa, y_2 would be included as an explanatory variable in the structural equation for y_1; y_1, in turn, is an explanatory variable in the structural equation of y_2. Estimation of this system of equations is possible, provided it is identified – i.e., rank and order conditions are met – using (inefficient) single equation methods (*indirect least squares, two-stage least squares, or LIML*) or (efficient) system methods (*three-stage least squares, FIML*).[14] Most econometric textbooks contain detailed discussions of the estimation of systems of equations (e.g., Greene, 2000).

In conclusion, we support Heckman's (2000) suggestion that it is sensible to think of endogeneity as the case where the *ceteris paribus* condition does not hold while manipulating one of the explanatory variables. Sources of endogeneity include omitted variables and simultaneity. Potential solutions for endogeneity following from both causes are available, but their success in applied settings varies greatly.

3. Omitted variables in the relation between cost-of-debt capital and disclosure

The previous section emphasized two main sources of endogeneity bias: (1) correlated omitted variables and (2) simultaneity. We will concentrate in the remainder of this paper on the first source because earlier literature has already investigated simultaneity bias in

[14] The tradeoff between single equation and system methods is that the latter are more susceptible to misspecification since they require the correct specification of all equations in the system. As an equivalent alternative one may estimate the reduced form of the structural model and then solve for the structural parameters in terms of reduced form parameters.

the relation between cost-of-capital and disclosure (Welker, 1995; Hail, 2002) and found that simultaneity bias does not appear to invalidate the results of OLS estimation.[15]

We first discuss (1) costs of disclosure[16] and (2) management reputation[17] as *examples* of unobservable firm characteristics that are likely correlated with disclosure and relatively fixed over time. Next, we review the literature in search of joint, observable determinants of both disclosure and cost-of-debt capital that were omitted in Sengupta (1998).

Unobservable firm characteristics

Costs of disclosure. While it is likely that the direct costs of disclosure (gathering and reporting information) differ between firms, some recent papers have focussed on a potentially interesting source of firm heterogeneity, i.e., the costs associated with investor uncertainty about the disclosure of information (Verrecchia, 2001). This uncertainty can originate from differences in technical expertise to understand the disclosure among the firm's investors (Fishman and Hagerty, 2003) or because it is unclear whether withholding disclosure results from firms having no information or having unfavourable information (Dye, 1985, 1998; Jung and Kwon, 1988). Whatever its origin, these models suggests that the extent of uncertainty affects the optimal disclosure policy of the firm. Intuitively, the firm may benefit from uncertainty because (unsophisticated) investors cannot distinguish between the two reasons for withholding information and, as a result, such investors may over value the firm.[18] The idea that investors differ in terms of their sophistication has found general

[15] We choose a research design that allows us to investigate endogeneity caused by omitted variables in relative isolation from endogeneity caused by simultaneity. We provide more details on this in Section 4. In short, we rely on the pre-determinedness of most of our RHS variables to argue that simultaneity is less likely to be severe. Nevertheless, we cannot exclude the possibility that simultaneity bias is present and our results should be interpreted with this caution in mind. One possible explanation why these earlier studies have not found that OLS is inconsistent might be that the instrument variables that were used in prior work were weak (see also, Larcker and Rusticus, 2005)

[16] Recent studies have pointed explicitly to the failure of many disclosure studies to take between-firm differences in costs of disclosure into account (Fields et al., 2001; Cohen, 2003).

[17] We would like to stress that these are indeed examples and many other reasonable theories exist. Agency costs are a clear alternative illustration. These costs are unobservable but likely differ among firms. Agency costs are likely to affect both the disclosure decision and the cost-of-capital. Yet another alternative is firm (as opposed to management) reputation. We do not aim at providing an exhaustive list of firm heterogeneity.

[18] See Hirshleifer and Teoh (2003) for a model in which pro forma disclosures are used to misdirect the attention of investors with limited cognitive abilities. To the extent that cognitive abilities among investors vary we expect different optimal levels of disclosure.

recognition in the empirical literature (Hand, 1990). Usually, sophistication is proxied by the proportion of institutional investors. Several papers document how capital market reactions differ depending on the composition of the firm's investor base (Kim et al., 1997; Walther, 1997, Bartov et al. 2000). Thus, the uncertainty of firms about the way the market will react to their disclosures is likely to differ. Not only will this uncertainty affect the optimal disclosure, but it will also affect cost-of-capital. Given that investors are uncertain about the nature of non-disclosure they need to be compensated in expected return. Therefore, both disclosure and costs of capital are affected by the unobservable firm-specific characteristic of the sophistication of investors.

Management reputation. Disclosure has been modelled as a device through which managers signal their talent (Trueman, 1986; Healy and Palepu, 2001). The reasoning usually is that more talented managers will reveal their type through making voluntary disclosures, although Nagar (1999) offers a model in which even talented managers may opt for non-disclosure in some cases. This author assumes that managers are differently talented and that they are uncertain about the market's response to the disclosure of their performance. Depending on the extent of the penalty the market puts on non-disclosing performance and the manager's discomfort from the uncertainty about the market's reaction to disclosure, the optimal disclosure policy will vary. Regardless of the supposed chain of events, managerial talent or discomfort are unobservable sources of firm heterogeneity.

It seems very likely that a manager's talent also affects the cost-of-debt capital. For example, more talented managers might make more persuasive propositions when seeking debt capital. Investors will consider the default risk of firms managed by talented managers to be lower. Their road shows should be more interesting to investors and they might attract bigger crowds eager to jump on the bandwagon of a talented manager and his or her firm. In sum, both cost-of-debt capital and disclosure are influenced by the manager's talent, and talent is likely to differ between firms but is also relatively constant over time in any one firm.

Joint determinants of disclosure and cost-of-debt capital

Lang and Lundholm (1993) suggest three categories of variables that will impact on the disclosure decision (1) performance variables, (2) structure variables, and (3) offer variables. These categories are motivated by theoretical arguments in which disclosing information reduces adverse selection problems between investor and firm, decreases transaction costs associated with trading on capital markets and limits potential litigation costs caused by withholding information relevant to investors. Each of these variables will likely also affect the firm's cost-of-debt capital. We will briefly discuss each category in turn and indicate its effect on disclosure and cost of capital.

It is well recognized that performance is related to disclosure, albeit that the exact nature of the relation between the two is complex (Miller, 2002). Some theoretical models (e.g., Verrecchia, 1983 and Lanen and Verrecchia, 1987) suggest that firms will withhold negative news but disclose positive news, a concern that is often voiced by regulators as well (see, e.g., Levitt, 1998). The empirical evidence so far is not consistent with these contentions, as some authors have shown that bad news is rushed forward to avoid legal action (Skinner, 1994, 1997), to warn investors about earnings disappointments (Kasznik and Lev, 1995) or to improve the conditions surrounding stock option grants (Aboody and Kasznik, 2000). Nevertheless, the evidence suggests that disclosure is associated with performance.

Firms that perform well are likely to meet more favourable conditions when vying for capital. Investors perceive firms with sustained superior performance as less risky or they attribute better prospects to these firms. Performance will therefore be negatively associated with the cost-of-debt capital.

Structure variables refer to the economies of scale in producing information and to the extent of information asymmetry between investors and firm. One structural variable is the size of the firm; the idea is that larger firms will have comparatively lower (accounting) costs to produce the same amount information than smaller firms. Larger firms will thus disclose more information.

The adverse selection problem between the firm and its investors will be larger when information asymmetry between the two parties is greater (Healy and Palepu, 2001; Dye, 2001; Diamond and Verrecchia, 1991). Since disclosure is an instrument to reduce information asymmetry, disclosure will be more extensive when information asymmetry (prior to disclosure) is perceived to be substantial.

As large firms are generally thought to be less risky, size is expected to be negatively associated with cost-of-debt capital (Fama and French, 1992, 1993). Similarly, information asymmetry increases the (default) risk an investor is exposed to when providing capital to a company (Amihud and Mendelson, 1986; Easley and O'Hara, 2004). The cost-of-capital is therefore increasing in the extent of information asymmetry.

Finally, the last category of factors that impact on the disclosure decision refers to the offer variable. Theory suggests that managers who consider making capital market transactions have incentives to disclose information to reduce information asymmetry problems (Myers and Majluf, 1984). Lang and Lundholm (1993, 1996) and Healy et al. (1999) find evidence consistent with this idea for equity and debt offerings, respectively and Frankel et al. (1995) for both.[19]

The extent of a firm's capital market transactions may also affect its cost-of-capital because the market may interpret the frequency of these transactions as a signal about the firm's performance (Myers and Majluf, 1984). For example, frequent, sizable public debt issues may change the market's assessment of the default risk of the firm. Offerings are therefore likely to be associated with the cost-of-debt.

In conclusion, we have described 1) some unobservable firm characteristics (costs of disclosure and management reputation) that are correlated with the firm's disclosure policy and 2) joint determinants that are likely to impact on both disclosure and cost-of-capital. When omitted from the analysis of the relation between cost-of-capital and disclosure, the

[19] Lang and Lundholm (2000) on the other hand provide evidence that increasing disclosure prior to a seasoned equity offering may be interpreted as 'hyping' the stock and firms experience continued negative returns subsequent to the offering announcement. This effect is probably difficult to witness in our sample since we do not have a continuous measure of disclosure policy, but instead rely on annual assessments of disclosure. See also, Jog and McConomy (2003) and Mak (1996)

results are likely to be misleading. In the following sections, we document the severity of the bias in analyses that do not incorporate unobservable firm characteristics or joint determinants of disclosure and cost-of-capital and suggest a methodology to mitigate the bias.

3. Research design and variable definitions

We start the analysis by replicating Sengupta's (1998) results on the relation between disclosure and cost-of-debt capital. Specifically, we estimate the following regression equation using Ordinary Least Squares:

$$YIELD_{it+1} = Intercept + \beta_1 Disclosure_{it} + \sum \beta_i Control_i + \varepsilon_{it} \qquad (1)$$

where

YIELD = The effective yield to maturity at the moment of a public bond issue. This is our measure of the cost-of-debt capital. Yield to maturity is defined as the discount rate that equates the current value of all future interest and principal payments to the capital provided by the lender at the moment of the bond issue.

Disclosure = Joint label for our four measures of corporate disclosure policy: (1) PCTRNK, the percentage rank of overall corporate disclosure policy, (2) PCTREL, the percentage rank of investor relations disclosure policy, (3) PCTANL, the percentage rank of disclosure through the firm's annual report and (4) PCTOPB, the percentage rank of quarterly and other publications disclosures. Percentage ranks are constructed from the assessment of corporate disclosure policy by the AIMR Corporate Information Committee in their Annual Reviews of Corporate Reporting Practices.[20] Percentage ranks for each disclosure measure are computed by ranking each firm from 1 to N within each industry, such that N is assigned to the firm with the highest AIMR disclosure score, etc. Subsequently, each firm's rank is divided by the total number of firms rated within its industry to obtain the percentage ranks.

[20] These ratings have been frequently used in earlier disclosure studies and are discussed in some detail elsewhere (Lang and Lundholm, 1996; Healy and Palepu, 2001; Core, 2001).

Control = These measures include leverage, coverage of interest expense, return-on-sales, the log of total assets, volatility of firm performance, the size of the bond issue, the issue's time to maturity, the call option properties of the security, the interest on constant maturity US treasury bills, the time-series variation in risk premium over that contained in treasure bills, and dummy variables for convertible bonds and subordinate debt. These controls intend to take into account firm and issue specific factors as well as macroeconomic circumstances. For brevity we refer the reader to Sengupta (1998) for a further justification of their inclusion in the analysis. Appendix A provides measurement details. Since it is our purpose to replicate Sengupta's findings and then investigate the potential endogeneity bias in the relation between cost-of-debt capital and disclosure, we defer discussion of these control variables.

The time subscripts are of importance. We measure cost-of-debt capital at t+1, while *Disclosure* and all control variables that are not bond issue specific are measured at t. We can therefore consider these right hand side variables as *predetermined*; although these variables may be contemporaneously (at t) determined jointly, with regard to future values (t+1) of cost-of debt capital they may be regarded as having already been determined (Greene, 2000). This is a common method to make plausible that innovations in the dependent variable are uncorrelated with the explanatory variables (i.e., to reduce the likelihood of simultaneity bias). Bond-issue specific controls are not predetermined and we cannot exclude the possibility that they are endogenous. Moreover, to the extent that autocorrelation is present, we can no longer assume that the disturbance term is uncorrelated with the explanatory variables. Results should be interpreted with this possibility in mind.

Next, we evaluate the importance of the first source of endogeneity bias in the OLS regression of Equation (1), i.e., the impact of omitted variables known to be a determinant of both cost-of-debt capital and disclosure policy. For this purpose, we augment Equation (1) with variables that intend to capture those categories listed in Lang and Lundholm (1993) and summarized above as joint determinants of disclosure policy and cost-of-capital. Specifically, we estimate the following equation using OLS:

$$YIELD_{it+1} = Intercept + \beta_1 Disclosure_{it} + \sum \beta_i Performance_i + \sum \beta_j Structure_j + \sum \beta_k Offer_k + \sum \beta_l Control_l + \varepsilon_{it} \qquad (2)$$

where

<u>Performance variables[21]:</u>

GROWTH = Average future growth in sales (item #12) between t+1 and t+3.

FROS = Average future return-on-sales (as defined earlier) between t+1 and t+3.

LOSS = Dummy variable that is unity for firms with negative current net income (item #18), and zero otherwise.

MTB = Market-to-book ratio at the end of the year, defined as market value of equity (item #24×item #25) divided by the book value of equity (item #60).

FROS×GROWTH = Interaction term between future return-on-sales and future growth rate. We include this variable to capture the potentially non-linear relation between performance and disclosure as suggested in Miller (2002). Before computing the interaction between FROS and GROWTH each of the variables is demeaned in order to make main effects interpretable.

<u>Structure variables:[22]</u>

CAPEXP = Capital expenditures in the current year (item #128) scaled by total assets (item #6). This variable captures information asymmetry about

[21] Sengupta (1998) includes two variables as control variables in his regression that would otherwise have been included into this category. These variables (current income and interest coverage) are therefore part of the specification of our Equation 1 as ROS and COVER, respectively.

[22] Sengupta (1998) includes the logarithm of total assets as a control variable in his regression. This variable (LASSETS) was therefore included as control in our Equation 1. Otherwise, it would have been included in the category of structure variables to proxy for the economies of scale in producing information.

	the firm's strategy and, in particular about its investment opportunities.
MOODRNK	= Moody's ranking of the firm's bond. MOODRNK equals 100 if the bond is rated A1 by Moody's and 1 if the bond has rating Caa1. MOODRNK declines linearly from 100 to 1. We include MOODRNK as a proxy for amount of information asymmetry between the firm and its investors. The idea is that high levels of information asymmetry will make the firm's securities more risky and will prompt Moody's to downgrade the firm's ranking (see, e.g., Bhojraj and Sengupta, 2003; Ziebart and Reiter, 1992; Kaplan and Urwitz, 1979; Fisher, 1959).[23]

<u>Offer variable:</u>

ISSUES	= Number of bond issues by firm i in the current year.

If omitted variables are a source of endogeneity bias in Equation (1) then including the variables described above will reduce the amount of bias and OLS estimation of the augmented equation should be consistent (in the absence of firm heterogeneity effects). Therefore we document changes in the coefficient estimate on *Disclosure* in Equations (1) and (2) to evaluate the extent of the endogeneity bias caused by omitted variables.

[23] The inclusion of MOODRNK as a determinant of cost-of-debt capital is contentious. While some prior studies have added credit ratings as a control variable (Mansi et al., 2003; Campbell and Taksler, 2003; Bagnani et al., 1994), other have not. Sengupta (1998) argues that credit rating agencies consider the quality of disclosure when deciding on a firm's credit rating. Including the rating alongside a measure of disclosure may therefore create multicollinearity problems and it might become difficult to separate out the effects of disclosure and of credit ratings. We decided to include MOODRNK not only because it is an established proxy of information asymmetry, but also because we believe it is important to try to establish if the market reacts to disclosure directly or to credit ratings which (indirectly) reflect disclosure quality. We have also conducted the empirical analyses without MOODRNK and we report these results in footnote 32. If MOODRNK is construed as a proxy for information asymmetry then a more appropriate measurement is before the firm discloses its information. Since MOODRNK is an issue-specific rating, it is not straightforward to implement this in the regressions. We check the robustness of our results to the timing of the measurement of information asymmetry by replacing MOODRNK by S&P long term debt rating (Compustat item 280), which is available for all firm-years in the sample. We use a lagged (t-1) value of this rating to ensure that it is measured before the disclosure at t. We report the results for this specification in footnote 32 as well.

Finally, we investigate both sources of endogeneity bias simultaneously. We use panel data techniques (fixed effects)[24] to estimate the following equation:

$$YIELD_{it+1} = Intercept + \beta_1 Disclosure_{it} + \sum \beta_i Performance_i + \sum \beta_j Structure_j + \sum \beta_k Offer_k + \sum \beta_l Control_l + \alpha_i + \varepsilon_{it} \quad (3)$$

where

α_i = Any unobservable firm-specific variable that remains fixed over time, and all other variables are as defined above.

Since the firm-specific variable α_i is assumed to remain constant, an alternative approach to fixed effects estimation is to re-specify Equation (3) in first differences and estimate it with OLS. Differencing provides researchers with an easy to implement solution to the heterogeneity bias (Wooldridge, 2002). Taking differences in Equation (3) will cause the firm-specific variable α_i to drop out of the equation. Note that differencing requires at least two consecutive years of data for each firm. We use first-differences estimation as a robustness check on our fixed effects findings.

Finally, we provide further evidence on the nature of the correlation, which theory suggests exists between *Disclosure* (as well as other independent variables) and the firm heterogeneity variable α_i using a procedure suggested by Mundlak (1978). We provide a brief and informal description of Mundlak's (1978) approach in Appendix B. Combined, the results for Equations 1-3 provide us with evidence on the magnitude of endogeneity bias caused by firm-specific heterogeneity and omitted variables. Note that while we focus on the effect of endogeneity on the coefficient on *Disclosure*, any of the RHS variables may

[24] In principle, Equation (3) could be estimated using fixed and random effects, respectively. The appropriateness of each estimator depends on assumptions about the correlation between α_i and the included independent variables. If the firm-specific characteristics captured in α_i are independent of the regressors, random effects estimation is consistent and efficient. However, if the firm-specific characteristics are correlated with any of the regressors this estimation procedure is inconsistent and fixed effects is preferred. Since we have strong theoretical reasons to believe that firm-specific characteristics are correlated with the disclosure variable, our priors are that fixed effects estimation is the most appropriate when estimating Equation 3. In fact, unreported results of a Hausman test of the consistency of random and fixed effects estimation support the choice for fixed effects. This is further evidence that firm heterogeneity is important in the current setting and should be taken into account (using fixed effects) when estimating the relation between disclosure and cost-of-debt capital.

(potentially) be correlated with the error term in the structural equation, and thus be endogenous. In fact, we show below this to be the case for CALL and RISK. To the extent that endogeneity is caused by time-invariant firm heterogeneity, the fixed effects estimation will alleviate the bias in all RHS variables.

Caveats.

The use of panel data techniques (especially, fixed effects or differencing) when multiple observations of a firm over time are available has become pervasive practice in the economics and finance literatures, although accounting researchers have been somewhat slow to emulate the example. This literature strongly demonstrates the importance of controlling for unobservable firm (or economic agent) heterogeneity in many settings.[25] Fixed effects estimation will, however, not always be successful in mitigating the problem of unobserved firm heterogeneity. Zhou (2001), for example, draws attention to the observation that if the relation under study is essentially a cross-sectional phenomenon, fixed effects estimation will not be effective. Indeed, since fixed effects estimation removes all cross-sectional (*between*) variation, one of its underlying assumptions is that over-time changes *within* each firm are driving the relation of interest. In the context of our setting, we need to establish that disclosure quality changes substantially over time for individual firms and that it is this within variation that impacts on cost-of-debt capital. Changes in disclosure should be indicative of substantive changes in disclosure policy. The next section provides evidence to underpin the validity of using fixed effects in our context.[26]

4. Sample and summary statistics

[25] Seminal studies include Mundlak (1961, 1978), Hoch (1962), Ben-Porath (1973), Griliches (1977), Ashenfelter (1978), Chamberlain (1978), Hausman (1978), Hausman and Taylor (1981). More recent applications in finance include Doidge (2004), Campbell and Taksler (2003), Himmelberg et al. (1999), Ashenfelter and Kruger (1994). In accounting, Francis et al. (2004), Hail and Leuz (2004) provide fixed effect results.

[26] Zhou (2001), Himmelberg et al. (1999) and Griliches and Hausman (1986) note that the fixed effect estimator may suffer from bias, which is associated with measurement error. Griliches and Hausman (1986) point out that measurement error will have a different impact on the fixed effects estimator and the first-differences estimator. Since we report fixed effects and first-differences results that are very close, it is unlikely that measurement error is a major issue here.

The sample comprises 358 firm-year observations from 100 firms during 1986-1996.[27] To be included in the sample, the firm needs to fulfil the following criteria: (1) public debt is issued during the sample period and data on yield-to-maturity and other issue characteristics are available on the SDC Platinum Database, (2) the firm's disclosure policy is rated by the AIMR, (3) accounting data is available on the CRSP/COMPUSTAT Merged Database and (4) future sales and earnings data is available to compute FROS and GROWTH. We excluded 80 firms with only one observation in the sample due to the requirements of the panel data techniques and we deleted firms in the financial industry. Table 1 documents the effect of each of the sample filters and breaks down the sample by year and by industry. The data necessary to compute the variables TBILL and RISKPR are taken from the Federal Reserve Database (FREDII).

Table 2 contains sample summary statistics. The average (median) value of our cost-of-debt capital measure [YIELD] is 8.14 (8.07), which is similar to Sengupta's (1998) findings. The average percentage rank of disclosure is (for all four measures) just above 0.5, indicating that our sample firms disclose more information than the average firm in their industry. The standard deviation of each disclosure score is about 0.27, which indicates that we have substantial disclosure variation in our sample. AIMR's disclosure ratings tend to focus on larger and better known firms. This bias is reflected in our sample since sample firms are large (mean (median) of total assets is $9.81($7.80) billion). Our sample is less skewed than Sengupta's who reports a mean (median) value of total assets of $10.1 ($6.02) billion.

Table 3 reports Pearson correlations (below the diagonal) and their p-values (above the diagonal). YIELD is significantly, negatively associated with three disclosure measures and negatively, but not significantly with the measure PCTOPB. The three specific disclosure

[27] Sengupta's (1998) sample consists of 103 observations (and as many firms, since he only retains one observations per firm). We have, due to our design, multiple observations for each firm, and consequently cannot claim that our observations are independent. To ascertain the extent of this problem we have compiled a sample in which each firm enters only once, and ran the benchmark model on this sample. Our results remained qualitatively unchanged and we conclude that any potential downward bias of the standard errors, due to dependent observations, is likely to be minor.

measures (PCTREL, PCTANL and PCTOPB) are positively and significantly associated with the overall measure of disclosure (PCTRNK), which suggests that disclosure practices via investor relations, the annual report and other publications are complementary.

We mentioned in the previous section that substantial over-time variation in each firm's disclosure quality is a precondition for applying fixed effects estimation. We conduct a first analysis of whether our sample fulfils this precondition in Table 4. The table contains the year-to-year transition probabilities matrix, which shows the probability of a firm moving from decile *i* in year t (shown in the first column) to decile *j* in year t+1 (shown in the first row). Panel A contains the transition matrix for entire AIMR sample (1986-1996). Panel B contains the transition matrix for our final sample. The findings suggest that the final sample is representative of the entire AIMR population. More importantly, the probability of *staying in the same* disclosure quality category from year to year generally does not exceed 25% (diagonal entries in each panel). Therefore, about 75% of firms either improve or worsen their disclosure over time. It would seem that the *within* variation is substantial and fixed effects estimation should be appropriate in the current setting. We address the requirement of substantial over-time variation in the firm's disclosure quality further in the *Additional Analysis* section.

5. Results

Benchmark model. Table 5 contains the results from pooled OLS regressions of Equation (1) for each of the four measures of *Disclosure*. These regressions replicate and extend Sengupta's original analysis. As in Sengupta (1998, Table 6)[28], we find a negative and strongly significant association (coefficient=-0.33, s.e.=0.12)[29] between the measure of overall disclosure policy (PCTRNK) and cost-of-debt capital. We also consistently find negative and significant associations between the three other measures of *Disclosure* (PCTREL, PCTANL and PCTOPB) and cost-of-debt capital. Note that this finding is somewhat in contrast with Botosan and Plumlee (2002) who report that the sign of the

[28] Note that the magnitudes of our coefficients are not directly comparable to those in Sengupta (1999) because our variable definitions are sometimes different.
[29] Standard errors throughout the paper are White (1980) heteroskedasticity consistent.

relation between disclosure and cost-of-capital is conditional on the type of disclosure (i.e., through investor relations, the annual report or other publications). Although not the focus of our attention, we find that most control variables are significant in all four regressions and have the same sign as in Sengupta (1998). Together the independent variables have good explanatory power; the adjusted R-squared is about 84%.

Main findings. We investigate the endogeneity bias caused by omitted 'joint determinants' in Tables 6 through 8. Recall that our claim is that Sengupta's model omits several variables theory suggests are correlated with both disclosure and cost-of-debt capital. We first evaluate whether these 'joint determinants' are indeed associated with *Disclosure* in Table 6 – Panel A. We report on regressions of each of our four *Disclosure* measures on those variables suggested in earlier literature, including Performance, Structure and Offer variables. The results show that all joint determinants (except for LOSS, LASSET and MTB) are significantly associated with our overall measure of *Disclosure*, PCTRNK. Although the results for the other three measures (PCTREL, PCTANL and PCTOPB) are somewhat mixed, we conclude that the complete set of variables has significant explanatory power for each *Disclosure* measure.[30] Table 5 – Panel B shows the results of an ANOVA analysis of the four *Disclosure* measures. We find that allowing firm-specific intercepts to explain disclosure accounts for much more of the variation in each of the *Disclosure* measures than our complete set of 'joint determinants' (the adjusted R-squared in the ANOVA analysis averages about 60% versus 9% in the regressions of Panel A). Our interpretation of this finding is that unobserved firm-specific factors are a very important consideration in explaining differences in disclosure policy. In addition, these results indicate that augmenting the benchmark model with the joint determinants alone may not suffice to eliminate the endogeneity bias in the results, if in fact unobserved firm heterogeneity is correlated with cost-of-debt capital.

Table 7 contains the results of the OLS estimation of the augmented Sengupta model, Equation (2) for each of the four *Disclosure* measures. These regressions only attempt to

[30] The simple correlations in Table 2 between each of the 'joint determinants' (and their best linear combination) and our *disclosure* variables are low and there is little reason to be concerned about multicollinearity being an issue in our subsequent analyses (see also Griffiths et al. 1993).

mitigate the endogeneity bias caused by omitted joint determinants. The Performance, Structure and Offer variables we included based on the extant literature are generally associated with cost-of-debt capital. The weakest results are obtained for FROS, the interaction FROS*GR, CAPEXP and ISSUES, which do not obtain significance in any of the four regressions. However, GROWTH, MTB and MOODRNK (LOSS) are strongly (marginally) associated with cost-of-debt capital. An F-test on the incremental explanatory power of all Performance, Structure and Offer variables together suggests that these variables are helpful in explaining cost-of-debt capital (in the overall disclosure measure regression, PCTRNK, F=10.31, p-value<1%)[31]. We find that *Disclosure* and cost-of-debt capital are no longer significantly associated once these 'joint determinants' are included in the regression. Note that the loss of significance is due to a reduced magnitude of the OLS coefficient on *Disclosure* compared with Equation (1) and not because of an increase in the standard errors and thus lack of power. From comparing these results with those of Equation (1), it would seem that in the latter equation *Disclosure* subsumes part of the effect of the joint determinants on cost-of-debt capital, which results in an upward bias of the coefficient on *Disclosure* in Sengupta's original model.[32]

Table 8 contains the findings for the fixed effects estimation of the augmented Sengupta model, i.e., Equation (3) for each of the *Disclosure* measures.[33] These regressions attempt to simultaneously control for firm-specific heterogeneity bias and for endogeneity caused by omitted variables. The findings are consistent throughout the table. Cost-of-debt capital is strongly negatively associated with disclosure policy *at the level of the individual firm*. The coefficient estimates range between -0.22 and -0.40 for each of the four *Disclosure* measures. In particular, we find that the fixed effect coefficient in Equation (3) on PCTRNK is -0.40 (s.e.=0.13) compared with the OLS coefficient in Equation (1), which is –0.33. The

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[31] The (unreported) results for the other three disclosure measures are similar to those for PCTRNK.
[32] We also estimated the model without MOODRNK. Unreported results show that in the augmented OLS regressions *Disclosure* remains significant, but the size of the coefficient is smaller than in a model without any control variables included. Replacing MOODRNK by the lagged value of S&P's long term debt rating did not affect the main findings and our conclusions remained unchanged.
[33] Random effects estimates for PCTREL, PCTANL and PCTOPB are available from the authors upon request.

implication is that the cost-of-debt capital benefit from increased disclosure is larger than previously reckoned. For a median size debt issue of \$149.8 million, an improvement of disclosure score from the 25th to the 75th percentile, may reduce interest payments by about \$10.4 million.[34]

So far, while we have directly documented the effect of omitted 'joint determinants', we have only indirectly shown that unobservable firm-specific factors exist that are associated with both cost-of-debt capital and disclosure. When these unobservable factors remain unaccounted for, the disclosure variable will subsume part of their effect on cost-of-capital. In such case, the reported association between cost-of-debt capital and disclosure is a mixture of the true association between these variables and a spurious part due to not accounting properly for unobservable firm-specific factors. We use Mundlak's (1978) approach to investigate directly how unobservable firm-specific factors are associated with disclosure (or other independent variables). Table 10 holds the results of this analysis for all four *Disclosure* measures. We find that our measure of overall disclosure (PCTRNK), disclosure via investor relations (PCTREL), and marginally disclosure via annual reports (PCTANL) and other publications (PCTOPB) are positively associated with unobservable firm-specific factors.[35] Note that several of the control variables in Sengupta's original model are also related with these firm-specific factors (especially, RISK and CALL), which reinforces the need for taking these effects into account when investigating the relation between cost-of-debt capital and disclosure.

These results confirm the presence of endogeneity bias and imply that firms with higher cost-of-capital levels are also the firms that happen to disclose more information. This occurs not because disclosure is causally related to cost-of-capital, but because both variables

[34] It should be noted, however, that the incremental explanatory power of the *Disclosure* variable is small (and below 1%). This is not unexpected though, since our model already explains almost 90% of the variation in cost-of-debt capital. What is more, the incremental explanatory power of *Disclosure* is of similar magnitude as our leverage variable, which is always very significant. Therefore, we believe that adding *Disclosure* to the model is meaningful regardless of its low incremental explanatory power.

[35] We also used feasible generalized least squares to estimate the relation between unobservable firm-specific factors and disclosure and our results (not reported, but available on request) were qualitatively similar and did not change our conclusions.

are driven by omitted factors. The resulting endogeneity bias works against finding a relation in the cross-sectional OLS regressions we report in Table 7. As such, our results offer an explanation why some earlier studies fail to find a relation between cost-of-capital and disclosure.

Based on these findings, we evaluate the bias in Sengupta's model by comparing the fixed effects estimation of the coefficient on disclosure in Equation (3) with the OLS estimation of the same coefficient in Equation (1). While the difference between the two estimates is sizable at about 21%, this number does not fully convey the magnitude of the bias in Equation (1). Considering our earlier analyses together, the biases caused by firm heterogeneity and by omitted variables are of opposite sign, partially cancelling each other out *in this specific setting.*

Additional Analyses. To show that our results do not depend on the specifics of fixed-effect estimation we also use OLS to estimate Equation (3) in first differences. The additional data requirement of two consecutive years of data reduces the number of firm-year observations to 258. The results (reported in Table 9) show that the coefficient on each of our *Disclosure* measures is similar in magnitude to the fixed effects estimates. We also tested whether our results are sensitive to using unadjusted ('raw') AIMR disclosure scores and whether the relation between disclosure and cost-of-debt capital is different for firms that increase vs. decrease disclosure over time. Our results do not change when using raw disclosure scores[36] and we do not find differences for firms with increasing or decreasing over time disclosure.

Finally, we reported transition probabilities in Table 4 and argued that the amount of within-firm variation is sufficient to warrant fixed-effects analysis. At the same time, however, since many firms appear to be changing from one disclosure quality decile to another, these changes may not reflect the necessary *substantial* changes in disclosure policy. Theory (e.g., Verrecchia, 2001) emphasizes that cost-of-capital effects are mainly expected when a firm commits to a higher standard of disclosure (as opposed to a transitory change in disclosure quality in any given year). Any ex-ante commitment to a specific disclosure quality

[36] Indeed, signs and significance remain similar in all cases except for the regressions of PCTANL.

will translate into a systematic component of disclosure quality and this component will be eliminated in the fixed effects estimation.[37] If we were to take theory literally, we should not find a cost-of-debt capital effect after removing the systematic component of disclosure via fixed effects estimation. Our main findings, however, indicate that the changes in our *Disclosure* metric are such that they have a cost-of-debt capital effect. Our metric apparently captures substantial disclosure policy changes. On the other hand, since so many firms change disclosure policy (in Table 4), one might ask if this interpretation is reasonable. Sceptics may argue that if disclosure policy changes happen this often, ex-ante commitment is a rather hollow concept.[38] We therefore consider next disclosure quality changes that are more exceptional (than movements to adjacent deciles) and which are more likely to capture disclosure *policy* changes. We conduct the following analyses to provide some evidence on this issue. We create disclosure quality deciles based on the sample of all AIMR firms (as in Table 4, Panel A). We then retain only those pairs of observations in the sample for which it is more likely that they reflect a change in the firm's commitment to a disclosure policy. Specifically, we retain two consecutive observations if a firm is grouped in decile k first and subsequently is grouped in decile $k \pm i$ where $i \geq 2$. Thus, the new sample contains only those observations where the firm 'jumps' over adjacent disclosure quality deciles. This restriction results is a final sample of 68 firms with 182 observations. We then run our main analysis again on this sample of firms with disclosure *policy* changes. Table 11 holds the details. As expected, we continue to find that disclosure policy affects the cost-of-debt capital. As before, OLS estimation of the augmented model produces an insignificant coefficient on *Disclosure*, but after adjusting for firm heterogeneity this coefficient is about twice larger than in the OLS

[37] Indeed, this is precisely why we use fixed effects estimation. The decision to commit to a disclosure policy is likely to be part of a portfolio of simultaneous firm choices on strategy, business profile, risk and environmental segments, compensation, and customer/supplier relates policies (Core, 2001). As such, the systematic component is likely to be endogenous and should be eliminated from the analysis.

[38] One alternative explanation for our findings could be that our *Disclosure* measure captures mostly random noise or performance-related variation in disclosure quality (either because good performance leads to better disclosure or because its leads to better *perceived* disclosure). Noise will attenuate the regression coefficient, but the performance part can induce a negative relation between disclosure and cost-of-debt capital. While the performance control variables should control for this, the net effect could still be a negative observed relation between disclosure and cost of capital.

regressions and strongly significant. We conclude from this that our original findings are similar to the findings for a sample of firms for which we can be more certain that they changed their disclosure policy. Interpreting the original findings as evidence for what happens if a firm changes its commitment to a certain disclosure policy would, consequently, not seem unreasonable.

6. Discussion and conclusion

Theory prescribes the following steps to address endogeneity. First, researchers should develop a theoretical model for the choice being examined. Next, researchers should determine which variables are considered exogenous in the setting under study and a reduced form model should be derived. Given that the model is identified, the reduced form can be estimated and the structural parameters can be recovered. This prescription appears to be ignored in many empirical studies. In particular, the requirement to formulate explicitly the underlying model for the choice being examined is, in our observation, seldom met in practice. Such model does not have to be formal, but should be based on a rigorous survey of what is known about the choice under investigation. Only once the underlying model is made explicit can the econometric properties of the estimated results be understood.

We argue that our understanding of the relation between cost-of-capital and disclosure is precarious because of the existence of an endogeneity bias in extant work. We investigate two important sources of endogeneity bias, (1) unobservable firm heterogeneity and (2) observable omitted variables. Theory suggests that firm heterogeneity may arise due to differences in costs of disclosure between firms or because management reputation varies among firms. Cost of disclosure as well as management reputation impacts on both cost-of-debt capital and disclosure. Neither is directly observable to the researcher and when omitted from the empirical analysis causes endogeneity bias. Earlier empirical and theoretical work has suggested that variables reflecting firm performance, structure and offerings are related to disclosure policy. These variables also affect cost-of-debt capital. Similar as before, when omitting these variables from the analysis an endogeneity bias is likely to arise.

We investigated how each of these two endogeneity biases affect the estimation of the relation between cost-of-debt capital and disclosure and documented substantial effects for both, albeit that firm heterogeneity appears to be the more important one. It also appears that in the current setting the two sources of bias are of opposite sign, which makes the net effect underestimate the true magnitude of the bias. We further investigate firm heterogeneity and show that disclosure is positively and significantly associated with unobservable firm-specific factors that cause heterogeneity. This reinforces our claim that the association between disclosure and cost-of-debt capital is partially driven by the disclosure variable reflecting omitted firm-specific factors.

We attempt to mitigate endogeneity bias by relying on theory to identify additional variables correlated with both disclosure and cost-of-debt capital and by applying fixed effects estimation. Fixed effects estimation is only expected to be helpful if the relation of interest between two variables is driven by changes over time within the firm. The relation under investigation should not be a cross-sectional phenomenon, since *between* variation is eliminated in the fixed effects approach. Empirically, we show that in our setting over-time changes in firm disclosure are substantial, which speaks to the fact that the relation between disclosure and cost-of-debt capital is surely not just a cross-sectional attribute. This finding is substantiated by the results of the fixed effects estimation, which demonstrate that after removal of the cross-sectional variation, a strong association exists between disclosure and cost-of debt capital. Implicitly, fixed effects estimation assumes that the changes in our disclosure measure are an indication of *substantive* changes in disclosure policy. Some theoretical studies suggest that cost-of-capital effects are expected to be most strongly when a firm commits to a certain level of disclosure *ex ante.* Since such commitment would lead to a relatively constant level of disclosure over time for any one firm, its effect would be subsumed by the variable α_i and drop out in the fixed effects estimation. In contrast, we established a strong relation between cost-of-debt capital and disclosure in the fixed effects estimation which is consistent with (1) changes in our disclosure measure being indicative of substantive changes in (ex ante commitment to) disclosure policy – and therefore not subsumed in α_i , or (2) changes in disclosure matter even after controlling for a firm's overall ex ante commitment to a specific level of disclosure. The latter explanation assumes that ex ante commitment to disclosure is not the only way to obtain cost-of-capital effects (see for a similar opinion: Dye, 2001). Earlier empirical work seems to concur. Healy et al. (1999) and Lundholm and Myers (2002), for example, show that changes in disclosures impact on stock return and stock liquidity. While we readily concede that the burden of proof is on the researcher to make sure that fixed effect estimation is appropriate in a specific setting to address endogeneity, we also believe that in our setting it clearly is a helpful method to mitigate at least some of endogeneity's confounding effects.

Based on our findings, we recommend that researchers collect multiple observations for each firm in their sample and use either a first-differences specification and OLS or fixed effects estimation to address the endogeneity bias in the relation between cost-of-debt capital and disclosure. Without explicitly accounting for endogeneity in this relation, any causal inference is likely to be fraught with problems.

Some may argue that using fixed effects estimation to address endogeneity in this or other settings is too simple a solution for a complex problem. Perhaps this is true, but at a minimum researchers should be warned that some concern is warranted if they find that OLS results change dramatically after the inclusion of fixed-effects. If nothing else, fixed effects may function as a crude diagnostic that the findings need additional scrutiny.

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Appendix A. Variable definitions.

YIELD	= The effective yield to maturity at the moment of bond issue.
PCTRNK	= The percentage rank of overall corporate disclosure policy.
PCTREL	= The percentage rank of investor relations disclosure policy.
PCTANL	= The percentage rank of disclosure through the firm's annual report.
PCTOPB	= The percentage rank of quarterly and other publications disclosures.
GROWTH	= Average future growth in sales (item #12) between t+1 and t+3.
FROS	= Average future return-on-sales (see, below) between t+1 and t+3.
LOSS	= Dummy variable that is unity for firms with negative current net income (item #18), and zero otherwise.
MTB	= Market-to-book ratio at the end of the year, defined as market value of equity (item #24×item #25) divided by the book value of equity (item #60).
FROS×GROWTH	= Interaction term between future return-on-sales and future growth rate.
CAPEXP	= Capital expenditures in the current year (item #128) scaled by total assets (item #6).
MOODRNK	= Moody's bond rating converted into the linear scale.
ISSUES	= Number of bond issues by firm *i* in the current year.
LEV	= Leverage, defined as long-term debt (Compustat item #9) divided by total assets (Compustat item#6).
COVER	= Coverage of interest expenses, a measure of the firm's ability to meet its debt service requirements, computed as income before extraordinary items and interest expense (item#18+item #15) divided by interest expense (item #15).
ROS	= Return-on-Sales, as a measure of the firm's operating performance, computed as earnings before interest, taxes, depreciation and amortization (item #13) divided by sales (item #12).
ASSET	= Total assets (item #6).

LASSET	= Log of total assets, to proxy for the size of the firm. Computed as the logarithm of total assets (item #6).
RISK	= Volatility of the firm's performance, defined as the firm's highest stock price in year t (item #23) minus the firm's lowest stock price in year t (item #22) divided by the end-of-year stock price (item #24).
SIZE	= Size of the bond issue in millions of dollars. This is the amount of capital received by the borrower.
TTM	= Time to maturity.
CALL	= The callability of the security, ranging between zero and unity. It the bond is callable form the moment of issue CALL equals unity. CALL is zero for non-callable securities. CALL is computed as the bond's maturity minus the time from the moment that the bond first becomes callable divided by the bond's time to maturity.
CONVER	= Bond convertibility. Dummy variable that takes the value of unity if the bond is convertible and zero otherwise.
SUBOR	= Bond subordination. Dummy variable that takes the value of unity for subordinate debt and zero otherwise.
TBILL	= Interest on constant maturity US treasury bonds. These bonds are matched with treasury bills by maturity. A time weighted average is computed if the maturity of the bond does not match with that of the treasury bill.
RISKPR	= Measure of the time-series variation in risk premium over that contained in TBILL. Defined as the difference between the yield on a Moody's Aaa bond and a treasury bill with 30 years maturity.

Appendix B. Mundlak's (1978) approach

In the random effects framework, a fundamental assumption is that the firm-specific effects are treated as strictly exogenous to present, future and past values of explanatory variables (Hsiao, 2003). Mundlak (1978) criticized the random effects specification precisely because there is usually very little reason to assume that firm-specific effects α_i are uncorrelated with the regressors explicitly controlled for. If one neglects such correlation the inferences are incorrect. Mundlak (1978) relaxes the assumption of strict exogeneity by allowing the individual effects to depend linearly on the average values of individual-specific means of the explanatory variables. Specifically:

$$y_{it} = \beta + \beta_1 x_{1it} + \ldots + \beta_k x_{kit} + \alpha_i + \varepsilon_{it} \qquad [M]$$

$$\alpha_i = \kappa_1 \overline{x}_{1i.} + \ldots + \kappa_k \overline{x}_{ki.} + \omega_i \qquad [Auxiliary\ regression]$$

where $\overline{x}_{1i.},\ldots,\overline{x}_{ki.}$ are average values of regressors for each individual *i*.

The coefficients $\kappa_1,\ldots,\kappa_k$ capture the extent of the correlation between the explanatory variable and the error term α_i. Mundlak demonstrated that the GLS vector of coefficients $[\kappa_1,\ldots,\kappa_k]$ is equal to the following difference: $\hat{\beta}^{between} - \hat{\beta}^{within}$, where $\hat{\beta}^{between}$ is a vector of slope coefficients from the regression where individual specific means in the dependent variable $\overline{y}_{i.}$ are regressed on the individual specific means in the independent variables $\overline{x}_{1i.},\ldots,\overline{x}_{ki.}$; and $\hat{\beta}^{within}$ is the Fixed Effects estimator. Moreover, Mundlak (1978) showed that GLS vector of coefficients in model *M* given the *auxiliary regression* equals the Fixed Effects estimator. On these grounds he claimed that there is only one correct estimator, which is the F.E. estimator.

Under the null hypothesis of no endogeneity $\hat{\beta}^{between}$ and $\hat{\beta}^{within}$, are independent and it is easy to construct test statistics in order to test the significance of $\kappa_1,\ldots,\kappa_k$ coefficients. We use a simple t-test: $Tstat = \dfrac{\hat{\beta}_k^{between} - \hat{\beta}_k^{within}}{\sqrt{Var\left(\hat{\beta}_k^{between}\right) + Var\left(\hat{\beta}_k^{within}\right)}}$.

TABLE 1
SAMPLE CHARACTERISTICS

PANEL A
Sampling Procedure

Subsample	**# firms**	**# of Obs.**
AIMR rated companies (1986-1996)	932	4705
i. AIMR companies in COMPUSTAT/CRSP	778	
ii. AIMR rated companies that issued debt	508	1604
i. and ii. Companies Merged (by year)	331	892
Net of Non-Industrial companies	237	604
After deletion of missing values	180	438
Companies with more than one observation	100	358

PANEL B
Distribution of the Number of times a given firm appears in the sample

# of times	**# Of Firms**	**# of Obs.**	**%**
2	35	70	19.6
3	23	69	19.3
4	17	68	19.0
5	9	45	12.6
6	9	54	15.1
7	5	35	9.8
8	1	8	2.2
9	1	9	2.5
Total:	100	358	100

PANEL C

Number of companies used in the analysis by year

YEAR	**# of Obs.**	**%**
1986	17	4.75
1987	13	3.63
1988	26	7.26
1989	29	8.10
1990	68	18.99
1991	52	14.53
1992	52	14.53
1993	19	5.31
1994	35	9.78
1995	32	8.94
1996	15	4.19
Total:	358	100.00

(Table 1: Continued)

PANEL D

Number of companies used in the analysis by Industry

INDUSTRY	# of Firms	# of Obs.	%
Aerospace	2	4	1.12
Airline	4	17	4.75
Apparel	1	7	1.96
Chemical	4	16	4.47
Construction	1	2	0.56
Container and Packaging	2	4	1.12
Diversified Companies	2	4	1.12
Domestic Oil	5	14	3.91
Electrical Equipment	4	11	3.07
Food, Beverage and Tobacco	17	48	13.41
Health Care	9	35	9.78
Independent Oil	2	5	1.40
International Oil	1	5	1.40
Machinery	3	13	3.63
Natural Gas Distributors	2	9	2.51
Natural Gas Pipeline	6	30	8.38
Nonferrous and Mining	2	5	1.40
Paper and Forest Products	12	47	13.13
Precious Metals	1	2	0.56
Publishing and Broadcasting	4	15	4.19
Railroad	3	12	3.35
Retail Trade	11	47	13.13
Specialty Chemicals	1	4	1.12
Textiles	1	2	0.56
Total:	100	358	100

TABLE 2
DESCRIPTIVE STATISTICS

Variable	Mean	St. Dev.	75th pct	Median	25th pct
YIELD	8.138	1.331	9.125	8.065	7.105
PCTRNK	0.578	0.284	0.824	0.632	0.375
PCTREL	0.559	0.271	0.793	0.598	0.360
PCTANL	0.571	0.278	0.806	0.618	0.375
PCTOPB	0.548	0.285	0.800	0.585	0.308
LEV	0.240	0.104	0.313	0.238	0.173
COVER	4.372	5.340	4.925	2.952	1.868
ROS	0.173	0.087	0.209	0.159	0.114
ASSETS	9817	11766	12130	7801	3000
LASSET	8.747	0.967	9.403	8.962	8.006
RISK	0.394	0.172	0.458	0.361	0.275
SIZE	179.2	123.5	225.0	149.8	99.7
LMATUR	16.293	11.193	30.000	10.000	10.000
CALL	0.174	0.308	0.300	0.000	0.000
CONVER	0.036	0.187	0.000	0.000	0.000
SUBOR	0.034	0.180	0.000	0.000	0.000
TBILL	7.311	1.017	8.110	7.340	6.570
RISKPR	0.669	0.126	0.760	0.650	0.590
MOODRNK	72.302	28.236	94.737	84.211	36.842
GROWTH	1.068	0.089	1.109	1.055	1.017
FROS	0.170	0.085	0.211	0.158	0.110
MTB	2.755	2.175	3.148	2.039	1.386
CAPEXP	0.087	0.049	0.110	0.076	0.054
FROSXGR	0.000	0.007	0.002	0.000	-0.002
LOSS	0.056	0.230	0.000	0.000	0.000
ISSUES	2.251	2.405	3.000	2.000	1.000

Table provides summary statistics for the variables used in subsequent analyses. The sample includes 100 companies, which amount to 358 firm-year observations. In order to avoid double counting we use only the first debt issue in a given year to measure YIELD. Bond attributes including YIELD are forwarded by one year since regressions use period t+1 debt issues when looking at period t disclosures. Disclosure score used to construct percentage rankings (PCTRNK, PCTREL, PCTANL, PCTOPB) are collected from AIMR-FAF reports over the period 1986-1996. The firm-level control variables are taken from CRSP/COMPUSTAT Merged database; debt issues information is taken from SDC Platinum Database; Macroeconomic variables come from FRED II. See Appendix A for variable definitions.

TABLE 3

PEARSON CORRELATIONS (BELOW DIAGONAL) AND THEIR SIGNIFICANCE LEVELS (ABOVE DIAGONAL)

	YIELD	PCTRNK	PCTREL	PCTANL	PCTOPB	LEV	COVER	ROS	LASSET	RISK	SIZE	LMATUR	CALL	CONVER	SUBOR	TBILL	RISKPR	MOODRNK	GROWTH	FROS	MTB	CAPEXP	FROSXGR	LOSS	ISSUES
YIELD		0.04	0.10	0.00	0.16	0.00	0.00	0.01	0.20	0.00	0.34	0.02	0.37	0.00	0.06	0.00	0.00	0.09	0.00	0.00	0.00	0.53	0.16	0.82	0.60
PCTRNK	-0.11		0.00	0.00	0.00	0.37	0.01	0.33	0.22	0.83	0.04	0.70	0.41	0.84	0.46	0.31	0.80	0.00	0.00	0.11	0.47	0.01	0.00	0.96	0.01
PCTREL	-0.09	0.74		0.00	0.00	0.80	0.03	0.22	0.04	0.24	0.01	0.33	0.76	1.00	0.34	0.40	0.63	0.01	0.01	0.15	0.10	0.07	0.00	0.27	0.00
PCTANL	-0.16	0.85	0.51		0.00	0.08	0.16	0.59	0.36	0.35	0.15	0.46	0.10	0.77	0.58	0.03	0.93	0.00	0.00	0.39	0.22	0.01	0.03	0.83	0.01
PCTOPB	-0.07	0.80	0.46	0.66		0.28	0.05	0.31	0.60	0.76	0.04	0.66	0.63	0.48	0.27	0.57	0.84	0.00	0.04	0.10	0.56	0.03	0.04	0.41	0.08
LEV	0.18	-0.05	0.01	-0.09	-0.06		0.00	0.15	0.88	0.08	0.89	0.37	0.94	0.16	0.79	0.97	0.16	0.00	0.02	0.19	0.00	0.12	0.13	0.00	0.01
COVER	-0.20	-0.14	-0.11	-0.08	-0.11	-0.55		0.00	0.80	0.37	0.05	0.11	0.98	0.84	0.56	0.23	0.78	0.01	0.01	0.00	0.00	0.09	0.04	0.00	0.10
ROS	-0.13	-0.05	-0.06	-0.03	-0.05	-0.08	0.23		0.04	0.02	0.08	0.19	0.80	0.87	0.40	0.21	0.81	0.92	0.12	0.00	0.00	0.00	0.55	0.00	0.30
LASSET	-0.07	0.06	0.11	0.05	0.03	-0.01	0.01	-0.11		0.00	0.00	0.61	0.02	0.00	0.00	0.04	0.00	0.25	0.00	0.12	0.10	0.00	0.21	0.12	0.01
RISK	0.21	0.01	-0.06	0.05	0.02	0.09	-0.05	-0.13	-0.21		0.39	0.93	0.35	0.11	0.12	0.01	0.02	0.01	0.05	0.00	0.01	0.00	0.18	0.44	0.80
SIZE	-0.05	0.11	0.14	0.08	0.11	0.01	0.11	-0.09	0.41	-0.05		0.11	0.86	0.27	0.99	0.22	0.14	0.78	0.67	0.03	0.00	0.01	0.27	0.76	0.05
LMATUR	0.13	-0.02	-0.05	-0.04	-0.02	0.05	0.08	-0.07	0.03	0.00	0.09		0.00	0.53	0.30	0.03	0.52	0.57	0.01	0.57	0.65	0.14	0.01	0.46	0.20
CALL	0.05	0.04	0.02	0.09	0.03	0.00	0.00	0.01	-0.12	0.05	-0.01	0.39		0.00	0.00	0.01	0.42	0.90	0.79	0.29	0.14	0.59	0.80	0.52	0.04
CONVER	-0.24	0.01	0.00	0.02	0.04	0.07	-0.01	-0.01	-0.24	0.09	0.06	0.03	0.40		0.00	0.02	0.34	0.03	0.11	0.80	0.75	0.01	0.00	0.12	0.06
SUBOR	-0.10	0.04	0.05	0.03	0.06	0.01	-0.03	-0.04	-0.17	0.08	0.00	0.05	0.23	0.54		0.10	0.74	0.00	0.13	0.36	0.87	0.94	0.02	0.67	0.09
TBILL	0.81	-0.05	-0.04	-0.12	-0.03	0.00	-0.06	-0.07	-0.11	0.13	-0.07	0.11	0.14	0.12	0.09		0.00	0.25	0.06	0.01	0.00	0.03	0.99	0.05	0.13
RISKPR	0.23	-0.01	-0.03	0.00	0.01	-0.07	0.02	0.01	-0.16	0.12	-0.08	-0.03	0.04	0.05	0.02	0.26		0.08	0.32	0.34	0.03	0.48	0.27	0.10	0.57
MOODRNK	-0.09	0.19	0.14	0.20	0.20	-0.35	0.14	-0.01	0.06	-0.14	-0.01	0.03	0.01	-0.12	-0.17	0.06	0.09		0.23	0.77	0.21	0.92	0.39	0.01	0.24
GROWTH	-0.20	0.16	0.15	0.18	0.11	-0.12	0.14	-0.08	-0.20	0.11	0.02	-0.14	0.01	0.09	0.08	-0.10	0.05	0.06		0.25	0.00	0.21	0.01	0.82	0.76
FROS	-0.20	-0.08	-0.08	-0.05	-0.09	-0.07	0.23	0.91	-0.08	-0.16	-0.11	-0.03	0.06	-0.01	-0.05	-0.14	-0.05	-0.02	-0.06		0.00	0.04	0.32	0.01	0.11
MTB	-0.36	0.04	0.09	0.07	0.03	-0.24	0.47	0.16	0.09	-0.14	0.22	-0.02	-0.08	-0.02	-0.01	-0.26	-0.11	0.07	0.22	0.23		0.51	0.26	0.25	0.53
CAPEXP	0.03	0.14	0.09	0.13	0.12	-0.08	0.09	0.17	-0.23	0.15	-0.14	-0.08	-0.03	0.14	0.00	0.11	0.04	-0.01	0.07	0.11	0.03		0.00	0.25	0.48
FROSXGR	0.07	-0.16	-0.17	-0.12	-0.11	-0.08	0.11	-0.03	0.07	-0.07	-0.06	0.14	-0.01	-0.19	-0.12	0.00	-0.06	0.05	-0.14	-0.05	-0.06	-0.24		0.22	0.75
LOSS	-0.01	0.00	-0.06	0.01	0.04	0.21	-0.18	-0.20	0.08	0.04	0.02	0.04	0.03	0.08	0.02	-0.10	-0.09	-0.14	0.01	-0.14	-0.06	-0.06	0.06		0.29
ISSUES	-0.03	0.15	0.16	0.14	0.09	0.13	-0.09	-0.06	0.36	-0.01	0.11	-0.07	-0.11	-0.10	-0.09	-0.08	0.03	0.06	0.02	-0.09	-0.03	0.04	-0.02	0.06	

Table reports Pearson correlations below the diagonal and their significance levels above the diagonal. Sample consists of 358 firm-year observations. See Appenidx 1 for variable definitions.

TABLE 4

YEAR-TO-YEAR TRANSITION PROBABILITIES MATRIX (FOR PCTRNK)

	Q 1	Q 2	Q 3	Q 4	Q 5	Q 6	Q 7	Q 8	Q 9	Q 10	All
Panel A: Entire AIMR sample (1986-1996)											
Q 1	**0.45**	0.22	0.13	0.06	0.04	0.04	0.02	0.01	0.02	0.01	**1.00**
Q 2	0.24	**0.31**	0.16	0.11	0.06	0.06	0.02	0.02	0.02	0.01	**1.00**
Q 3	0.11	0.17	**0.26**	0.16	0.08	0.07	0.05	0.04	0.03	0.02	**1.00**
Q 4	0.06	0.11	0.18	**0.22**	0.14	0.12	0.05	0.07	0.03	0.03	**1.00**
Q 5	0.05	0.06	0.13	0.16	**0.19**	0.15	0.10	0.06	0.06	0.03	**1.00**
Q 6	0.04	0.08	0.08	0.09	0.14	**0.19**	0.16	0.11	0.07	0.04	**1.00**
Q 7	0.01	0.02	0.06	0.09	0.10	0.16	**0.20**	0.17	0.11	0.07	**1.00**
Q 8	0.01	0.03	0.04	0.03	0.10	0.11	0.18	**0.21**	0.19	0.11	**1.00**
Q 9	0.01	0.02	0.04	0.03	0.05	0.09	0.12	0.19	**0.23**	0.22	**1.00**
Q 10	0.01	0.02	0.01	0.02	0.05	0.05	0.08	0.12	0.21	**0.46**	**1.00**
Panel B: Final sample (used to estimate our OLS/FE regressions) (1986-1996)											
Q 1	**0.42**	0.16	0.05	0.16	0.05	0.11	0.00	0.05	0.00	0.00	**1.00**
Q 2	0.14	**0.29**	0.36	0.14	0.07	0.00	0.00	0.00	0.00	0.00	**1.00**
Q 3	0.12	0.24	**0.24**	0.18	0.00	0.06	0.12	0.00	0.06	0.00	**1.00**
Q 4	0.00	0.16	0.16	**0.05**	0.21	0.21	0.16	0.05	0.00	0.00	**1.00**
Q 5	0.00	0.08	0.00	0.08	**0.15**	0.31	0.23	0.00	0.00	0.15	**1.00**
Q 6	0.00	0.00	0.06	0.11	0.11	**0.11**	0.11	0.17	0.33	0.00	**1.00**
Q 7	0.00	0.00	0.00	0.07	0.14	0.07	**0.50**	0.14	0.00	0.07	**1.00**
Q 8	0.00	0.00	0.00	0.05	0.16	0.05	0.11	**0.21**	0.26	0.16	**1.00**
Q 9	0.00	0.00	0.00	0.00	0.14	0.14	0.21	0.00	**0.29**	0.21	**1.00**
Q 10	0.00	0.00	0.11	0.00	0.00	0.00	0.00	0.22	0.33	**0.33**	**1.00**

Table contains the year-to-year transition probabilities matrix, which shows the probabilities of a firm moving from quantile i in year t (shown in the columns) to quantile j in year t+1 (shown in the rows). Panel A contains the transition matrix for the entire AIMR sample (1986-1996), which includes only firms with at least two consecutive observations (3624 firm-years). Panel B contains transition matrix for our final sample with at least two consecutive observations (156 firms).

TABLE 5

REPLICATION OF THE SENGUPTA'S FINDINGS FOR DIFFERENT DISCLOSURE MEASURES

$$YIELD_{it+1} = Intercept + \beta_1 Disclosure_{it} + \sum \beta_k Control_k + \varepsilon_{it}$$

Type of Discl.		(A) TOTAL RANK (PCTRNK)			(B) INV. RELAT. (PCTREL)			(C) ANNUAL (PCTANL)			(D) OTHER PUBL. (PCTOPB)		
Variable	*Sign*	*Coeff.*	*St. Dev.*	*P-value*	*Coeff.*	*St. Dev.*	*P-value*	*Coeff.*	*St. Dev.*	*P-value*	*Coeff.*	*St. Dev.*	*P-value*
Disclosue	**-**	**-0.332**	**0.122**	**[.007]**	**-0.292**	**0.102**	**[.005]**	**-0.318**	**0.133**	**[.017]**	**-0.196**	**0.115**	**[.090]**
LEV	+	2.170	0.338	[.000]	2.298	0.351	[.000]	2.174	0.337	[.000]	2.250	0.345	[.000]
COVER	-	-0.015	0.006	[.011]	-0.013	0.006	[.025]	-0.014	0.006	[.020]	-0.013	0.006	[.031]
ROS	-	-0.706	0.396	[.075]	-0.740	0.393	[.061]	-0.705	0.401	[.080]	-0.718	0.398	[.072]
LASSET	-	-0.099	0.043	[.021]	-0.098	0.043	[.024]	-0.098	0.043	[.022]	-0.102	0.043	[.018]
SIZE	+	0.001	0.000	[.025]	0.001	0.000	[.027]	0.001	0.000	[.031]	0.001	0.000	[.031]
RISK	+	0.732	0.215	[.001]	0.690	0.212	[.001]	0.756	0.222	[.001]	0.722	0.220	[.001]
LMATUR	+	0.000	0.003	[.880]	0.000	0.003	[.983]	0.000	0.003	[.965]	0.001	0.003	[.860]
CALL	+	0.353	0.138	[.011]	0.351	0.136	[.010]	0.373	0.143	[.010]	0.339	0.136	[.013]
CONVER	-	-2.971	0.320	[.000]	-2.983	0.326	[.000]	-2.972	0.322	[.000]	-2.960	0.333	[.000]
SUBOR	+	0.088	0.311	[.778]	0.102	0.317	[.748]	0.079	0.313	[.801]	0.082	0.327	[.803]
TBILL	+	1.069	0.028	[.000]	1.073	0.028	[.000]	1.063	0.029	[.000]	1.073	0.028	[.000]
RISKPR	+	0.395	0.273	[.150]	0.395	0.276	[.153]	0.405	0.274	[.139]	0.401	0.281	[.154]
C		0.392	0.523	[.454]	0.313	0.512	[.542]	0.402	0.534	[.451]	0.286	0.502	[.569]
Adj-R2		0.848			0.847			0.848			0.845		
NOB:		358			358			358			358		

Table provides estimates for Equation (1) using pooled OLS regressions. The model is an equivalent of that estimated by Sengupta (1998). Column A of the table replicates Sengupta's results using the measure of total disclosure quality (PCTRNK). The following three columns, respectively, use measures of quality of investor relations (PCTREL), annual reports (PCTANL), and quarterly and other publications (PCTOPB). All four measures of disclosure are constructed using AIMR-FAF disclosure scores for the period 1986-1996. The disclosure scores are converted to within industry percentage rankings in order to achieve better comparability across industries and over time: for each year and each industry the firms are ranked based upon disclosure score, then the rankings are divided by the number of firms being ranked. Sample includes 100 companies, which amount to 358 firm-year observations. In order to avoid double counting we use only first debt issue in a given year to measure YIELD. Standard errors are White heteroscedasticity consistent. See Appendix A for variable definitions.

TABLE 6
DETERMINANTS OF DISCLOSURE

PANEL A: OLS Estimation

$$Disclosure_{it} = Intercept + \sum \beta_k DETERMINANTS_k + \varepsilon_{it}$$

Type of Discl.		(A) TOTAL RANK (PCTRNK)			(B) INV. RELATIONS (PCTREL)			(C) ANNUAL (PCTANL)			(D) OTHER PUBL. (PCTOPB)		
Variable	*Sign*	*Coeff.*	*St. Dev.*	*P-value*	*Coeff.*	*St. Dev.*	*P-value*	*Coeff.*	*St. Dev.*	*P-value*	*Coeff.*	*St. Dev.*	*P-value*
C		-0.252	0.258	[.330]	-0.209	0.263	[.428]	-0.295	0.252	[.241]	0.028	0.268	[.916]
GROWTH	+	0.418	0.176	[.018]	0.362	0.170	[.034]	0.473	0.171	[.006]	0.238	0.179	[.186]
ROS	+/-	0.499	0.415	[.229]	0.089	0.383	[.817]	0.274	0.389	[.481]	0.548	0.434	[.207]
FROS	-	-0.722	0.408	[.078]	-0.357	0.375	[.342]	-0.382	0.405	[.346]	-0.786	0.437	[.073]
LOSS	+	0.039	0.074	[.598]	-0.060	0.072	[.407]	0.048	0.068	[.479]	0.098	0.070	[.162]
FROSXGR	-	-5.306	2.454	[.031]	-5.378	1.804	[.003]	-3.204	1.988	[.108]	-3.698	2.680	[.169]
MTB	+	0.001	0.007	[.860]	0.007	0.008	[.377]	0.003	0.007	[.645]	0.003	0.007	[.674]
LASSET	+	0.023	0.016	[.136]	0.030	0.016	[.059]	0.018	0.016	[.257]	0.009	0.017	[.583]
CAPEXP	+	0.656	0.276	[.018]	0.425	0.283	[.134]	0.651	0.273	[.018]	0.562	0.280	[.045]
MOODRNK	-	0.002	0.001	[.001]	0.001	0.001	[.028]	0.002	0.001	[.001]	0.002	0.001	[.000]
ISSUES	+	0.010	0.005	[.023]	0.011	0.005	[.023]	0.010	0.005	[.032]	0.006	0.007	[.400]
Adj-R2		0.096			0.082			0.081			0.065		
NOB:		358			358			358			358		

PANEL B: Analysis of Variance (ANOVA) of Disclosure quality proxies

		TOTAL RANK (PCTRNK)			INV. RELATIONS (PCTREL)			ANNUAL (PCTANL)			OTHER PUBL. (PCTOPB)		
Adj-R2		0.617			0.523			0.557			0.642		
H_0: $\alpha_I = \alpha$		F(99,258)	6.821	[0.0000]	F(99,258)	4.948	[0.0000]	F(99,258)	5.541	[0.0000]	F(99,258)	7.492	[0.0000]

In Panel A the determinants (*DETERMINAMTS*) for each of the four disclosure quality measures (PCTRNK, PCTREL, PCTANL, PCTOPB) are investigated. The main purpose of these regressions is to demonstrate that variables we identified as determinants of disclosure and classified into Performance, Structure and Offer groupings relate to the level of disclosure (in addition to the variables in Sengupta (1998)). Panel B reports F-statistics from an ANOVA analysis to demonstrate that firm-specific effects alone explain a larger proportion of variation in the disclosure proxies as the determinants in the regressions in Panel A. An F-test is used to test for the significance of the differences in firm-specific disclosure levels. The sample includes 100 companies and 358 firm-year observations. Standard errors are White heteroscedasticity consistent. See Appendix A for variable definitions.

TABLE 7
AUGMENTED MODEL ESTIMATED FOR FOUR DISCLOSURE MEASURES USING ORDINARY LEAST SQUARES

$$YIELD_{it+1} = Intercept + \beta_1 Disclosure_{it} + \sum \beta_i Performance_i + \sum \beta_j Structure_j + \sum \beta_k Offer_k + \sum \beta_l Control_l + \varepsilon_{it}$$

Type of Disclosure		(A) TOTAL DISCLOSURE			(B) INV REL. (PCTREL)			(C) ANNUAL (PCTANL)			(D) OTHER (PCTOPB)		
Variable	*Sign*	*Coeff.*	*St.Dev*	*P-value*	*Coeff.*	*St.Dev*	*P-value*	*Coeff.*	*St.Dev*	*P-value*	*Coeff.*	*St.Dev*	*P-value*
Disclosure	**-**	**-0.172**	**0.111**	**[.122]**	**-0.095**	**0.096**	**[.320]**	**-0.154**	**0.118**	**[.193]**	**-0.062**	**0.102**	**[.543]**
Performance													
GROWTH	-	-1.200	0.337	[.000]	-1.237	0.347	[.000]	-1.210	0.335	[.000]	-1.260	0.350	[.000]
FROS	-	-1.032	1.005	[.305]	-0.932	0.995	[.350]	-0.978	0.989	[.323]	-0.939	1.008	[.352]
LOSS	+	0.329	0.206	[.111]	0.319	0.208	[.126]	0.330	0.205	[.107]	0.332	0.204	[.105]
MTB	-	-0.047	0.014	[.001]	-0.048	0.014	[.001]	-0.048	0.015	[.001]	-0.049	0.015	[.001]
FROSXGR	+/-	1.051	3.873	[.786]	1.343	3.963	[.735]	1.403	3.952	[.723]	1.573	3.942	[.690]
Structure													
CAPEXP	-	0.484	0.831	[.561]	0.397	0.838	[.636]	0.460	0.838	[.583]	0.377	0.839	[.654]
MOODRNK	-	-0.005	0.001	[.000]	-0.005	0.001	[.000]	-0.005	0.001	[.000]	-0.005	0.001	[.000]
Offer													
ISSUES	-	0.007	0.011	[.510]	0.006	0.011	[.555]	0.007	0.011	[.528]	0.006	0.011	[.592]
Controls													
LEV	+	1.607	0.330	[.000]	1.664	0.334	[.000]	1.611	0.330	[.000]	1.645	0.331	[.000]
COVER	-	-0.002	0.005	[.644]	-0.001	0.005	[.908]	-0.001	0.005	[.776]	0.000	0.005	[.946]
ROS	-	0.138	1.042	[.895]	0.034	1.036	[.974]	0.094	1.029	[.928]	0.054	1.049	[.959]
LASSET	-	-0.135	0.046	[.003]	-0.136	0.046	[.003]	-0.135	0.045	[.003]	-0.138	0.046	[.003]
SIZE	+	0.001	0.000	[.004]	0.001	0.000	[.005]	0.001	0.000	[.005]	0.001	0.000	[.005]
RISK	+	0.563	0.205	[.006]	0.549	0.203	[.007]	0.576	0.210	[.007]	0.561	0.207	[.007]
LMATUR	+	-0.001	0.003	[.764]	-0.001	0.003	[.722]	-0.001	0.003	[.715]	-0.001	0.003	[.740]
CALL	+	0.386	0.124	[.002]	0.379	0.122	[.002]	0.393	0.129	[.002]	0.375	0.121	[.002]
CONVER	-	-3.044	0.294	[.000]	-3.038	0.297	[.000]	-3.041	0.296	[.000]	-3.030	0.297	[.000]
SUBOR	+	0.021	0.292	[.942]	0.015	0.297	[.960]	0.015	0.293	[.958]	0.009	0.296	[.976]
TBILL	+	1.050	0.028	[.000]	1.053	0.028	[.000]	1.047	0.029	[.000]	1.052	0.028	[.000]
RISKPR	+	0.454	0.267	[.091]	0.460	0.269	[.088]	0.462	0.267	[.084]	0.464	0.270	[.086]
C		2.581	0.787	[.001]	2.571	0.790	[.001]	2.593	0.792	[.001]	2.595	0.798	[.001]
Adj-R^2		0.872			0.872			0.872			0.871		

a. In addition to control variables in Sengupta's model (*Control*), the model includes three additional groupings of control variables: Performance, Structure and Offer. Performance captures the future prospects of the company. Structure captures information asymmetries between investors and the firm and the economies of scope in producing information. Offer measures the extent of capital market transactions. All three groups are related in theory to the level of disclosure and to YIELD.

b. Equation (2) is estimated using pooled OLS. The columns A-D of the table report on each of four disclosure quality proxies respectively: total disclosure quality (TOTRNK), quality of investor relations (PCTREL), quality of annual reports (PCTANL), and quality of quarterly and other publications (PCTOPB). All four measures of disclosure are constructed using AIMR-FAF disclosure scores for the period 1986-1996. The disclosure scores are converted to within industry percentage rankings in order to achieve better comparability across industries and over time: for each year and each industry the firms are ranked based upon disclosure score, then the rankings are divided by the number of firms being ranked. Sample includes 100 companies, which amount to 358 firm-year observations. In order to avoid double counting we use only first debt issue in a given year to measure YIELD. Standard errors are White heteroscedasticity consistent. See Appendix A for variable definitions

TABLE 8

AUGMENTED MODEL ESTIMATED FOR FOUR DISCLOSURE MEASURES USING FIXED EFFECTS

$$YIELD_{it+1} = Intercept + \beta_1 Disclosure_{it} + \sum \beta_i Performance_i + \sum \beta_j Structure_j + \sum \beta_k Offer_k + \sum \beta_l Control_l + \alpha_i + \varepsilon_{it}$$

Type of Disclosure		(A) TOTAL DISCLOSURE			(B) INV REL. (PCTREL)			(C) ANNUAL (PCTANL)			(D) OTHER (PCTOPB)		
Variable	*Sign*	*Coeff.*	*St.Dev*	*P-value*	*Coeff.*	*St.Dev*	*P-value*	*Coeff.*	*St.Dev*	*P-value*	*Coeff.*	*St.Dev*	*P-value*
Disclosure	**-**	**-0.400**	**0.130**	**[.002]**	**-0.377**	**0.118**	**[.002]**	**-0.348**	**0.134**	**[.010]**	**-0.223**	**0.133**	**[.094]**
Performance													
GROWTH	-	-0.575	0.410	[.162]	-0.685	0.412	[.098]	-0.636	0.416	[.128]	-0.713	0.425	[.095]
FROS	-	-0.192	1.153	[.868]	0.061	1.159	[.958]	-0.197	1.169	[.866]	-0.143	1.158	[.902]
LOSS	+	0.162	0.146	[.268]	0.121	0.152	[.425]	0.176	0.148	[.235]	0.167	0.153	[.276]
MTB	-	-0.020	0.024	[.402]	-0.014	0.025	[.564]	-0.021	0.025	[.394]	-0.023	0.024	[.341]
FROSXGR	+/-	5.123	4.713	[.278]	5.082	4.685	[.279]	5.043	4.728	[.287]	5.719	4.762	[.231]
Structure													
CAPEXP	-	0.410	1.044	[.695]	0.167	1.063	[.875]	0.382	1.047	[.716]	0.361	1.062	[.734]
MOODRNK	-	-0.001	0.003	[.697]	-0.001	0.003	[.809]	-0.001	0.003	[.653]	-0.001	0.003	[.620]
Offer													
ISSUES	-	0.007	0.012	[.565]	0.007	0.012	[.569]	0.007	0.012	[.540]	0.007	0.012	[.600]
Controls													
LEV	+	1.585	0.638	[.014]	1.682	0.646	[.010]	1.594	0.641	[.014]	1.703	0.651	[.010]
COVER	-	0.000	0.010	[.967]	0.001	0.010	[.918]	-0.005	0.010	[.604]	-0.002	0.010	[.797]
ROS	-	-3.021	1.172	[.011]	-3.204	1.193	[.008]	-2.896	1.163	[.013]	-3.110	1.180	[.009]
LASSET	-	-0.107	0.141	[.452]	-0.126	0.142	[.378]	-0.144	0.137	[.294]	-0.143	0.134	[.290]
SIZE	+	0.001	0.000	[.006]	0.001	0.000	[.004]	0.001	0.000	[.006]	0.001	0.000	[.006]
RISK	+	0.193	0.165	[.244]	0.167	0.160	[.296]	0.202	0.165	[.222]	0.179	0.164	[.277]
LMATUR	+	0.000	0.003	[.960]	-0.001	0.003	[.737]	0.000	0.003	[.939]	-0.001	0.003	[.739]
CALL	+	0.237	0.097	[.015]	0.257	0.098	[.009]	0.250	0.096	[.010]	0.239	0.097	[.014]
CONVER	-	-3.099	0.446	[.000]	-3.121	0.445	[.000]	-3.130	0.447	[.000]	-3.102	0.452	[.000]
SUBOR	+	0.237	0.364	[.515]	0.226	0.366	[.538]	0.239	0.370	[.519]	0.190	0.374	[.611]
TBILL	+	1.060	0.029	[.000]	1.068	0.029	[.000]	1.055	0.028	[.000]	1.062	0.028	[.000]
RISKPR	+	0.599	0.264	[.024]	0.595	0.266	[.026]	0.599	0.264	[.024]	0.642	0.270	[.018]
C													
Adj-R^2		0.920			0.920			0.919			0.918		

a. In addition to control variables in Sengupta's model (*Control*), the model here includes three additional groupings of control variables: Performance, Structure and Offer. Performance captures the future prospects of the company. Structure captures information asymmetries between investors and the firm and the economies of scope in producing information. Offer measures the extent of capital market transactions. All three groups are related in theory to the level of disclosure and to YIELD.

b. The columns A-D of the table report on each of four disclosure quality proxies respectively: total disclosure quality (TOTRNK), quality of investor relations (PCTREL), quality of annual reports (PCTANL), and quality of quarterly and other publications (PCTOPB). All four measures of disclosure are constructed using AIMR-FAF disclosure scores for the period 1986-1996. The disclosure scores are converted to within industry percentage rankings in order to achieve better comparability across industries and over time: for each year and each industry the firms are ranked based upon disclosure score, then the rankings are divided by the number of firms being ranked.

c. The displayed results are the estimates from Fixed Effects regression for Equation (3).

d. Sample includes 100 companies, which amount to 358 firm-year observations. In order to avoid double counting we use only first debt issue in a given year to measure YIELD. Standard errors are White heteroscedasticity consistent. See Appendix A for variable definitions.

TABLE 9

RELATIONSHIP BETWEEN DIFFERENT TYPES OF DISCLOSURE QUALITY AND COST OF DEBT: MODEL IN DIFFERENCES

$$\Delta YIELD_{it+1} = Intercept + \beta_1 \Delta Disclosure_{it} + \sum \beta_i \Delta Performance_i + \sum \beta_j \Delta Structure_j + \sum \beta_k \Delta Offer_k + \sum \beta_l \Delta Control_l + \varepsilon_{it}$$

Type of Discl.		**TOTAL RANK**			**RELATIONS**			**ANNUAL**			**OTHER**		
Variable	*Sign*	*Coeff.*	*St Dev*	*P-value*	*Coeff.*	*St Dev*	*P-value*	*Coeff.*	*St Dev*	*P-value*	*Coeff.*	*St Dev*	*P-value*
ΔDisclosure	**-**	**-0.418**	**0.144**	**[.004]**	**-0.379**	**0.128**	**[.004]**	**-0.393**	**0.130**	**[.003]**	**-0.168**	**0.140**	**[.233]**
ΔPerformance													
ΔGROWTH	-	-0.281	0.436	[.520]	-0.349	0.436	[.425]	-0.278	0.443	[.530]	-0.379	0.446	[.396]
ΔFROS	-	0.172	1.346	[.899]	0.592	1.315	[.653]	0.265	1.349	[.845]	0.341	1.351	[.801]
ΔLOSS	+	0.288	0.126	[.023]	0.239	0.130	[.068]	0.290	0.124	[.020]	0.278	0.138	[.044]
ΔMTB	-	-0.032	0.025	[.210]	-0.025	0.025	[.324]	-0.033	0.026	[.196]	-0.033	0.025	[.189]
ΔFROSXGR	+/-	11.485	5.862	[.051]	11.715	5.709	[.041]	11.595	5.741	[.045]	13.267	5.879	[.025]
ΔStructure													
ΔCAPEXP	-	1.580	1.235	[.202]	1.330	1.266	[.294]	1.673	1.244	[.180]	1.558	1.272	[.222]
ΔMOODRNK	-	0.001	0.003	[.645]	0.002	0.003	[.574]	0.001	0.003	[.717]	0.001	0.003	[.871]
ΔOffer													
ΔISSUES	-	0.007	0.015	[.629]	0.005	0.015	[.726]	0.008	0.015	[.596]	0.008	0.015	[.583]
ΔControls													
ΔLEV	+	1.048	0.751	[.164]	1.154	0.760	[.131]	1.074	0.757	[.157]	1.110	0.766	[.149]
ΔCOVER	-	-0.003	0.009	[.771]	-0.003	0.010	[.742]	-0.006	0.009	[.504]	-0.005	0.010	[.594]
ΔROS	-	-3.702	1.047	[.000]	-3.900	1.032	[.000]	-3.576	1.038	[.001]	-3.749	1.038	[.000]
ΔLASSET	-	0.284	0.199	[.155]	0.295	0.202	[.147]	0.260	0.206	[.207]	0.260	0.201	[.198]
ΔSIZE	+	0.001	0.000	[.088]	0.001	0.000	[.059]	0.001	0.000	[.077]	0.001	0.000	[.083]
ΔRISK	+	-0.311	0.285	[.275]	-0.336	0.280	[.231]	-0.333	0.280	[.236]	-0.329	0.288	[.255]
ΔLMATUR	+	0.003	0.003	[.433]	0.002	0.003	[.601]	0.003	0.003	[.410]	0.002	0.003	[.610]
ΔCALL	+	0.136	0.093	[.142]	0.172	0.093	[.065]	0.145	0.092	[.117]	0.158	0.092	[.087]
ΔCONVER	-	-2.897	0.361	[.000]	-2.927	0.358	[.000]	-2.910	0.358	[.000]	-2.906	0.365	[.000]
ΔSUBOR	+	0.295	0.316	[.351]	0.280	0.310	[.368]	0.319	0.324	[.325]	0.266	0.327	[.418]
ΔTBILL	+	1.077	0.033	[.000]	1.082	0.033	[.000]	1.064	0.031	[.000]	1.075	0.033	[.000]
ΔRISKPR	+	0.917	0.281	[.001]	0.932	0.280	[.001]	0.905	0.283	[.002]	0.995	0.289	[.001]
C		-0.030	0.042	[.468]	-0.042	0.042	[.317]	-0.036	0.043	[.399]	-0.038	0.043	[.380]
Adj-R2		0.872			0.871			0.872			0.868		
NOB:		258			258			258			258		

a. The Equation (3) model is estimated in differences. Differencing is alternative method to remove unobserved heterogeneity bias since the firm specific effects drop out from the model. The results are similar to Fixed Effects treatment. Column A reports the findings for the measure of total disclosure quality (PCTRNK).

Columns B-D report on measures of quality of investor relations (PCTREL), annual reports (PCTANL), and quarterly and other publications (PCTOPB). All four measures of disclosure are constructed using AIMR-FAF disclosure scores for the period 1986-1996. The disclosure scores are converted to within industry percentage rankings in order to achieve better comparability across industries and over time: for each year and each industry the firms are ranked based upon disclosure score, then the rankings are divided by the number of firms being ranked.

b. In addition to control variables in Sengupta's model (*Control*), the model here includes three additional groupings of control variables: Performance, Structure and Offer. Performance captures the future prospects of the company. Structure captures information asymmetries between investors and the firm and the economies of scope in producing information. Offer measures the extent of capital market transactions. All three groups are related in theory to the level of disclosure band to YIELD. Sample includes 100 companies, which amount to 258 firm-year observations. In order to avoid double counting we use only first debt issue in a given year to measure YIELD. Standard errors are White heteroscedasticity consistent. See Appendix A for variable definitions.

TABLE 10

AUXILIARY REGRESSION PROPOSED BY MUNDLAK (1978)

$$\alpha_i = \kappa_1 \overline{Disclosure_{it}} + \sum \kappa_i \overline{Performance_i} + \sum \kappa_j \overline{Structure_j} + \sum \kappa_k \overline{Offer_k} + \sum \kappa_l \overline{Control_l}$$

Type of Discl.		TOTAL RANK			RELATIONS			ANNUAL			OTHER		
Variable	*Sign*	*Coeff.*	*St Dev*	*P-value*	*Coeff.*	*St Dev*	*P-value*	*Coeff.*	*St Dev*	*P-value*	*Coeff.*	*St Dev*	*P-value*
PCTRNK		0.319	0.166	0.056	0.542	0.159	0.001	0.212	0.172	0.219	0.241	0.165	0.146
Performance													
GROWTH		-1.350	0.858	0.117	-1.327	0.862	0.125	-1.262	0.865	0.146	-1.236	0.872	0.157
FROS		-4.609	4.708	0.328	-4.564	4.692	0.332	-4.658	4.703	0.323	-4.529	4.726	0.339
LOSS		0.159	0.219	0.470	0.224	0.224	0.318	0.130	0.222	0.559	0.165	0.226	0.464
MTB		-0.021	0.025	0.398	-0.032	0.025	0.201	-0.019	0.025	0.453	-0.020	0.025	0.434
FROSXGR		-7.251	50.529	0.886	-6.822	50.387	0.892	-6.862	50.496	0.892	-7.794	50.670	0.878
Structure													
CAPEXP		-1.051	2.342	0.654	-1.126	2.353	0.633	-0.973	2.327	0.676	-1.129	2.329	0.628
MOODRNK		-0.004	0.003	0.134	-0.005	0.003	0.078	-0.004	0.003	0.161	-0.004	0.003	0.148
Offer													
ISSUES		0.001	0.013	0.948	-0.007	0.013	0.583	0.002	0.013	0.900	-0.002	0.013	0.871
Controls													
LEV		-0.651	0.958	0.497	-0.684	0.954	0.474	-0.677	0.957	0.480	-0.712	0.972	0.464
COVER		-0.006	0.010	0.556	-0.002	0.010	0.810	-0.001	0.010	0.945	-0.001	0.010	0.930
ROS		7.144	4.348	0.101	7.100	4.333	0.102	7.078	4.328	0.103	7.118	4.378	0.105
LASSET		0.001	0.146	0.995	0.024	0.147	0.873	0.040	0.141	0.776	0.040	0.139	0.776
SIZE		0.000	0.000	0.540	0.000	0.000	0.885	0.000	0.000	0.525	0.000	0.000	0.688
RISK		1.477	0.373	0.000	1.545	0.369	0.000	1.509	0.376	0.000	1.491	0.373	0.000
LMATUR		-0.002	0.003	0.428	-0.001	0.003	0.776	-0.003	0.003	0.370	-0.001	0.003	0.670
CALL		0.531	0.156	0.001	0.475	0.157	0.003	0.544	0.158	0.001	0.512	0.156	0.001
CONVER		0.263	0.626	0.674	0.360	0.625	0.565	0.288	0.625	0.646	0.293	0.632	0.643
SUBOR		-0.534	0.501	0.288	-0.544	0.503	0.281	-0.555	0.508	0.275	-0.489	0.512	0.341
TBILL		-0.065	0.036	0.067	-0.082	0.036	0.024	-0.066	0.034	0.058	-0.072	0.035	0.042
RISKPR		-0.160	0.667	0.810	-0.188	0.666	0.778	-0.131	0.668	0.844	-0.215	0.673	0.749

Table provides estimates of an auxiliary regression introduced by Mundlak (1978) and their significance levels based on t-test. An upper bar over the variables included in the model indicates the firm-specific averages of regressors. Test statistics constructed using heteroscedasticity consistent standard errors from Within and Between estimators (described in more detail in Appendix B) and using the fact that the latter and the former are independent under the null hypothesis of no misspecification. The results suggest a positive correlation between the error term and the dependent variable. See Appendix A for variable definitions.

TABLE 11

AUGEMENTED MODEL ESTIMATED USING OLS AND FIXED EFFECTS ON A SAMPLE OF FIRMS WITH SUBSTANTIAL DISCLOSURE POLICY CHANGES

$$YIELD_{it+1} = Intercept + \beta_1 Disclosure_{it} + \sum \beta_i Performanc\,e_i + \sum \beta_j Structure_j + \sum \beta_k Offer_k + \sum \beta_l Control_l + \alpha_i + \varepsilon_{it}$$

Estimator:		**OLS** (α_i=0)			**WITHIN (FIXED EFFECTS)**		
Variable	*Sign*	*Coeff.*	*St.Dev*	*P-value*	*Coeff.*	*St.Dev*	*P-value*
PCTRNK	**-**	**-0.188**	**0.181**	**[.300]**	**-0.324**	**0.149**	**[.032]**
Performance							
GROWTH	-	-0.997	0.439	[.025]	-0.732	0.552	[.188]
FROS	-	-1.607	1.323	[.226]	-1.520	1.747	[.387]
LOSS	+	0.531	0.286	[.065]	0.177	0.204	[.389]
MTB	-	-0.041	0.024	[.093]	-0.048	0.030	[.115]
FROSXGR	+/-	-1.281	4.470	[.775]	-1.869	8.806	[.832]
Structure							
CAPEXP	-	-0.175	1.130	[.877]	-1.156	1.627	[.479]
MOODRNK	-	-0.656	0.001	[.000]	-0.973	0.003	[.781]
Offer							
ISSUES	-	0.041	0.032	[.198]	0.040	0.039	[.308]
Controls							
LEV	+	1.342	0.432	[.002]	1.537	1.162	[.189]
COVER	-	-0.242	0.010	[.804]	-0.497	0.020	[.807]
ROS	-	1.275	1.314	[.333]	-3.786	1.804	[.039]
LASSET	-	-0.212	0.070	[.003]	-0.218	0.197	[.273]
SIZE	+	0.001	0.000	[.141]	0.001	0.000	[.051]
RISK	+	0.395	0.282	[.164]	-0.218	0.199	[.277]
LMATUR	+	-0.176	0.004	[.648]	0.001	0.004	[.778]
CALL	+	0.509	0.177	[.005]	0.137	0.138	[.322]
CONVER	-	-2.806	0.434	[.000]	-2.776	0.233	[.000]
SUBOR	+	-0.756	0.407	[.065]	-0.434	0.481	[.369]
TBILL	+	1.046	0.039	[.000]	1.057	0.040	[.000]
RISKPR	+	0.787	0.379	[.039]	1.198	0.368	[.002]
C		3.024	1.007	[.003]			
Adj-R2		0.857			0.942		

This table reports the results of OLS and fixed effects estimation of Equation (3), but restricts the sample to observations with substantive changes in disclosure. Substantive changes are defined as cases when a firm moves between two consecutive observations from disclosure quality decile k to decile $k \pm i$, where i is greater or equal to 2. The sample consists of 68 firms with 182 firm-year observations. Deciles are formed on the entire set of companies ranked by AIMR in a given year.